THE MEDICAL NEWS.

A WEEKLY JOURNAL OF MEDICAL SCIENCE.

VOL. LXV. SATURDAY, JULY 14, 1894. NO. 2.

ORIGINAL ARTICLES.

THE REACTIONS OF NUCLEO-ALBUMIN (ERRONEOUSLY STYLED MUCIN) WITH THE COMMONLY EMPLOYED URINARY ALBUMIN-TESTS: THE DIFFICULTY OF DISTINGUISHING THESE REACTIONS FROM THOSE OF SERUM-ALBUMIN, GLOBULIN, Etc.

Remarks on the Occurrence of a Normal, Constant Trace of Albumin in the Urine.[1]

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IN a paper[2] read before the Society for Clinical Research, I drew casual attention to certain fallacies attending the use of delicate tests for the detection of serum-albumin in urine, laying stress on one especial source of error which I regarded as the most important and misleading. This related to the frequent presence normally of nucleo-albumin in amount sufficient to react to nearly all of these tests, such as Tanret's, Millard's, and Sebelein's reagents, and to all other similar test-solutions containing tannin, mercury, or a vegetable acid. This also applied to picric acid, especially the citrated solution, and to metaphosphoric acid, but particularly to trichloracetic acid, now much in vogue as a reagent for the detection of small amounts of serum-albumin in the urine.

I pointed out that a reaction could be so commonly obtained from several of these, with the urine of the healthy, that unless this were regarded, it were unquestionably better in clinical work to depend upon the less misleading, if also less delicate time-honored methods, especially that by ebullition, with, if necessary, the addition of acetic acid.

I had at one time regarded the slight reaction so frequently obtained with healthy urine, as indicative of the existence of a physiologic albuminuria, but within the past year or two the result of certain experiments of my own and the work of others in the same field has rather tended to convince me that in the vast number of cases the substance reacting to the very delicate tests is a mucoid body probably originating from the cellular elements of the extra-renal passages. My reasons for so believing I have entered into elsewhere,[1] so need not here detail them. Lastly, re-experimenting with the delicate tests, in investigating certain cases of non-albuminuric nephritis, I have been especially interested to determine if the nature of the reaction often obtained by several of the very refined of these, and presumably not due to serum-albumin or to globulin, could be exactly determined, and whether it be due to a mucoid body, settled.

Exact experiments with various percentages of artificially added nucleo-albumin to albumin-free urine seemed the most satisfactory mode of ascertaining with certainty the nature of its reactions, and their differences from those of serum-albumin, serum-globulin, the albumoses and peptone, bodies with which there is no doubt that nucleo-albumin has repeatedly been generally confused by many of the most accurate and perhaps all of the careless workers in urinalysis. As stated in the paper previously referred to, urinary nucleo-albumin, which is now regarded as identical with the nucleo-albumin of the bile, is not true mucin, though still improperly so termed by many who are unaware of the important differences in these bodies which separate them both chemically and physically. Briefly, these bodies are distinguished in that concentrated solutions of mucin are mucilaginous and thready; with acetic acid mucin gives a precipitate insoluble in excess, while mucin, as a glycoproteid substance, after prolonged ebullition with dilute mineral acids yields, besides an albuminous body, a carbohydrate which reduces copper hydroxid.[2] Magnesium sulphate, also, is without precipitating effect upon solutions of mucin, totally unlike the case with solutions of nucleo-albumin. These differences in reaction have shown that it is doubtful if true mucin is ever present in the urine, and that the reacting body is in reality nucleo-albumin. This fact is of the highest clinical importance, as apart from a local nucleo-albuminuria,[3] all the in-

[1] Read before the College of Physicians of Philadelphia, May 2, 1894.

[2] "A Serious Fallacy attending the Employment of Certain Delicate Tests for the Detection of Serum-albumin in the Urine, especially the Trichloracetic-acid Test," THE MEDICAL NEWS, May 5, 1894.

[1] American Journal of the Medical Sciences, December, 1893. THE MEDICAL NEWS, April 14, 1894. Ibid., May 5, 1894.

[2] See Hammarsten: Treatise on Physiological Chemistry. Sheridan Lea: The Chemical Basis of the Human Body. Neubauer and Vogel: Analyse des Harns. Gamgee: Physiological Chemistry of the Animal Body.

[3] That is, nucleo-albumin derived from the cells of some part of the urinary tract.

dications point to the fact that a nucleo-albuminuria, the nucleo-albumin of which is underived from any portion of the urinary passages, is of no uncommon occurrence. The clinical aspect of nucleo-albuminuria cannot now be discussed; suffice it here to state that it is a subject worthy of attention, and the study of which is in the line of the elucidation of not a little that is obscure concerning the question of albuminuria without nephritis.

In the course of recent experiments with urine from the healthy, specimens of day and food urine were often encountered which, with proper precautions to exclude precipitation of urates, gave a sharp cloud after appropriate treatment with acetic acid; but it was always found impossible to obtain by filtration, although dilution was first practised,[1] sufficient of a precipitate in this manner to be utilized for experiment, such really small amounts did the precipitate represent.

As urinary nucleo-albumin is unobtainable in trade the experiments were made with nucleo-albumin (Merck's) derived from bile. This substance is regarded as identical, both chemically and in point of its various reactions, with nucleo-albumin occurring in urine. This body was found to be very slightly soluble in distilled water, and much more soluble in neutral or in alkaline saline solutions. With none of these in concentrated solution did it have a mucinous appearance, and the other tests, including ebullition with a dilute mineral acid, and subsequent testing of the precipitate for a reducing substance, showed that the body contained no true mucin. In the solid state it gave the xanthoproteic reaction, but reacted doubtfully to Adamkiewicz's reagent, and gave no response to Millon's, Liebermann's, or to the biuret test. This last was also examined for in strong solution, without result. This lack of reaction to certain of these color-reagents is interesting not only as a point in separating the nucleo-albuminous response from that due to other albuminous bodies, but also in indicating that the nucleo-albumin employed in the experiments made by Müller,[2] in which he obtained all the color-reactions, was contaminated with a second albuminous body.

[1] Because of the soluble effects of certain of the urinary salts, it is more easy to remove the mucoid precipitate produced by acetic acid in dilute urine than in one of ordinary gravity. It is also on this principle that the interesting fact pointed out by Reisser is explained, that if to a specimen of urine which my be clouded by the addition of acetic acid, a second is added which is not so affected, no cloud may be produced by testing the mixture.

[2] See Neubauer and Vogel (loc. cit.). It is there stated that Müller obtained all the color-reactions with nucleo-albumin. The varying results there given can only be so explained. It is stated also that one observer noted that heat was without effect upon a solution of nucleo-albumin, and a second the contrary. The experiments I detail were made in a manner precluding error of this sort.

Acting upon the fact that neutral salts, such as NaCl, AmCl, Na_2SO_4, and sodium acetate, serve to maintain the nucleo-albumin of the urine in liquescence, no difficulty was encountered in obtaining a perfectly clear solution of this substance. The diluents used for the experiments were saline solutions of distilled water and normal acid urine; the former was prepared to contain approximately the amount of mineral salts, represented by NaCl, and a less amount of sodium acetate, that exist in urine of ordinary gravity. No allowance was made for urea, and this is regarded as being without auxiliary solvent effect. Specimens of very clear, filtered, morning urine from normal individuals were employed which, unfiltered, gave no reaction with trichloracetic acid until after five minutes' contact. Nucleo-albumin was added in proportions varying from approximately 0.4 per cent. to 0.02 per cent. In the preparation of the solutions this substance was first triturated with a small quantity of the diluent, subsequently thoroughly agitated with the total amount, and on each occasion thoroughly boiled for some minutes.[1] After a large number of filtrations a clear solution could be obtained. When an amount upward of 0.4 per cent. was added, some slight residue always remained on the filter; with smaller portions solution of the whole quantity was readily obtained, although filtration in removing turbidity naturally coincidently withdrew a certain amount in fine suspension.

A very large number of experiments, approaching several hundred, were made with these solutions. A variety of tests in common use for the detection of albumin in urine were employed: ebullition with and without the addition of acetic acid; nitric acid; acetic acid and potassium ferrocyanid; metaphosphoric acid; plain and acidulated saturated solution of picric acid; Millard's reagent; trichloracetic acid, and a test-solution of mercuric chlorid with tartaric acid, a newly introduced reagent regarded of greater delicacy than Tanret's solution. Besides the employment of the color-reagents, notably the biuret test, the behavior of magnesium and ammonium sulphates in neutral solution was also ascertained.

The reactions with acetic and citric acids should be first mentioned. Citric acid was employed in both saturated and dilute solution. With these in all the various strengths of nucleo-albumin solution experimented with, a sharp contact-ring was ob-

[1] Lest ebullition have any decomposing effect upon the nucleo-albumin, altering its character, which, however, was not anticipated, in the form of menstruum employed, all the experiments detailed with the boiled solutions were repeated with solutions of similar strength prepared without heat. The results were identical. This likewise showed the absence of contamination by ordinary albumin, more than traces of which would, of course, be removable by boiling.

tained with a more or less diffused haze above. Acetic acid was used in the form of the liquid glacial acid, and in 2 per cent. and also in 25 per cent. strengths. The reaction with acetic acid was similar to those obtained with urines containing the mucoid body. The acid was applied both by the contact-method and by the addition of small amounts to 10 c.c. or more of the test-urine. A contact-ring was obtained with all strengths of acid, with a cloud above, though a less marked contact-reaction occurred with the undilute glacial acid, in which latter, indeed, nucleo-albumin can itself be dissolved. A cloud occurred instantly on the addition of small amounts of acid to the body of a few centimeters of test-solution, its intensity and rate of appearance varying directly with the amount of nucleo-albumin present, and inversely with the proportion of salts present.

A reaction occurred with HNO_3, employed by the contact-method, with all strengths of nucleo-albumin solutions. It was still apparent as a slight contact-cloud in a dilution giving but a trifling haze with plain picric acid. With solutions approximately 0.1 per cent. or more, though carefully underlaid by the acid with a capillary pipet, the whole stratum of solution soon became opaque, and after standing a short time a deeper, opaque ring was found at the point of contact of the two liquids. With less strengths than 0.1 per cent., such as from 0.05 to 0.08, it was easy to obtain an instantaneous contact-reaction quite like the ordinary serum-albumin ring, although it lacked the appearance of denseness; the latter, if of any depth, usually has little or no opacity above, with these strengths of solution so tested.

A slight contact-cloud was still apparent with the weakest solution used, approximating 0.2 per cent., a dilution which gave but a faint contact-haze with unacidulated picric acid after standing a few moments. A few drops of fuming HNO_3 added to a few centimeters of urine also produced a slight opacity in urine containing this same dilution of nucleo-albumin.

A combination of acetic acid and potassium ferrocyanid, a test for serum-albumin in the urine first proposed by Pavy, and now much in use, gave the ordinary albumin-reaction, entirely indistinguishable from that of serum-albumin.[1] This reaction was still apparent in the least strength tested. The reaction was decided at 0.05 per cent., and was still apparent at 0.025 per cent.

The various strengths of nucleo-albumin solutions also reacted similarly to metaphosphoric (glacial) acid. This reaction was in no way distinguishable from that of serum-albumin.

With considerable amounts of nucleo-albumin present, the whole solution above the piece of glacial acid became almost instantly opaque. With small amounts an almost instant contact-opacity occurred, typically such as is produced by serum-albumin, and gradually diffused from the portion of acid throughout the fluid. If but traces of nucleo-albumin were present, but still sufficient to react to acetic acid, though scarcely appreciable with unacidulated picric acid, the contact-response was delayed, not appearing for from two to three minutes, but soon becoming decided with longer contact.

A decided reaction was obtained with a saturated solution of picric acid, and with the citrated solution, indistinguishable in appearance from the contact-response with ordinary serum-albuminous urine. The ring was most sharply defined when the nucleo-albumin solution was of not too low a gravity (above 1010). No reaction, save a tardily appearing contact-haze, occurred, however, to unacidulated picric acid with a nucleo-albuminous dilution approximating 0.02 per cent., although the citrated picric acid still gave a ring, and a response was still evident to metaphosphoric acid, and very markedly to trichloracetic acid, to Millard's, and to the mercuric test.

I had before shown that unacidulated picric acid solution reacted to the urinary mucoid body,[1] which is contrary to the view of Sir George Johnson. These experiments finally settle the question.

With Millard's reagent nucleo-albumin solution continued to give a sharp contact-reaction with a dilution at which plain picric acid had entirely ceased to react.

With trichloracetic acid up to the weakest dilution of nucleo-albumin employed, a sharp contact-reaction was obtained. With a dilution approximating from 0.025 to 0.04, with which unacidulated picric acid gave no more than a contact-haze, a markedly broad, opaque ring was apparent with trichloracetic acid.

With the tartrated mercuric reagent a similar reaction was obtained as that of the others, although it was found that this reagent did not compare in delicacy with trichloracetic acid.

It is now of importance to study in greater detail the reactions of nucleo-albumin with these albumin-tests, and to determine, if possible, how they may be differentiated from the reactions of serum-albumin, serum-globulin, the albumoses, and peptone, but especially from the first named. It will be recalled

[1] Even so careful a chemist as Hammarsten (Text-book of Physiological Chemistry) does not speak of the similarity of reaction of nucleo-albumin and serum-albumin to either metaphosphoric acid or to the reagent of acetic acid and potassium ferrocyanid, in recommending these latter for the detection of albumin. No writer refers to confusion arising from the employment of metaphosphoric acid.

[1] By the removal of the reacting body with acetic acid in the cold. See American Journal of the Medical Sciences, December, 1893; THE MEDICAL NEWS, April 14, 1894.

that as thorough ebullition was invariably practised with the acid artificial or natural urine in preparing the nucleo-albuminous solutions, more than traces of ordinary albumin could scarcely be present; and that as it was necessary to repeatedly filter to obtain a clear solution for testing, more than traces of the albumoses, or so-called urinary peptones, if present, would also be removed; as filtration of the solution was repeatedly done not only while hot but after it was thoroughly cooled.

The likeness of nucleo-albumin to urinary albumose or peptone is striking in several particulars. That the one has in the past often been mistaken for the other, I have no doubt, in view of the identity in many of the reactions of the two. This curious similarity in reaction of these bodies probably depends upon a close kinship. As Wright[1] and others have shown, nucleo-albumin is identical with cell-fibrinogen, which under the influence of ferments (proteolytic) is decomposed into a nuclein-moiety and albumose, or so-called peptone, the last of which is not infrequently present in the urine in cases of active leucolysis. It is conceivable and probable that traces of the albumoses are also present in the urine in conditions leading to proteolysis in the bladder of the urinary nucleo-albumin, through the action of pepsin and trypsin, which are normally present in the urine, and by the aid as well of formed enzymes. But that nucleo-albumin is not identical with the albumoses or peptone is shown by its ready precipitation by acetic and other of the vegetable acids, and by the fact, as I have ascertained, that it will not give the biuret-reaction. This last seems a very positive mode of differentiating it from the other albuminous bodies.[2]

Its differentiation from ordinary peptone is quite easy, as I have found that by saturating hot urine containing nucleo-albumin with ammonium sulphate, all traces of nucleo-albumin are removed, so that no response can be subsequently obtained with trichloracetic acid.

Its similarity in several points to serum-globulin is also quite striking. Its presence in the urine, I believe, has often been mistaken by those who are unaware that globulin very rarely, if ever, exists without the coincident presence of a greater amount of serum-albumin.[3] Like serum-globulin, if mucoid nucleo-albumin is present in some amount in the urine, as I have found is common in specimens of the day-urine (after food and exercise), it may not infrequently be precipitated from solution by considerable dilution of the urine with water, lessening the soluble effect of the urinary salts, or by dialysis to a very low specific gravity; or, like globulin, it may be readily thrown down by magnesium sulphate after neutralization with ammonia and separation of the earthy phosphates.

A strikingly interesting and important feature of the reactions of mucoid nucleo-albumin to the albumin-tests is the influence of heat. Although the presence of this body may be readily shown by the intelligent use of acetic acid or other of the vegetable acids, in the cold, yet without the assistance of heat it would be quite impossible by any reagent, were the reaction decided, save perhaps the biuret-test, to definitely assert that serum-albumin is not also present in some amount.[1] Heat with every test employed save two—the metaphosphoric acid, and acetic acid and potassium ferrocyanid—tends to diminish a nucleo-albumin reaction if the amount of this body present is in moderate or small percentage, such as is likely to be encountered in the urine. This result was invariable with a percentage upward of 0.15. With the presence of greater amounts, the soluble effect of heat was much less apparent; and with a percentage of from 0.3 to 0.4, heat was either without influence or slightly increased the reaction previously obtained. This effect, also, I have found varies in degree with different specimens of the diluent of approximately the same degree of acidity containing not only different strengths of nucleo-albumin, but the same amount. The cause of this variation seems largely due to the character and amount of salts present.[2] Albumin-free[3] urines, and artificial urines experimented with, showed this variation with different percentages of nucleo-albumin. The reagents, whether HNO_3, picric acid, trichloracetic acid, or others, seem to hold, after precipitation, a portion of the nucleo-albumin, so that a temperature approaching the boiling-point, even with very small percentages of nucleo-albumin present, will not entirely dissipate the precipitate.

[1] See a paper on "Tissue- or Cell-fibrinogen in its Relations to the Pathology of the Blood." A. E. Wright, Lancet, March 5, 1892.

[2] It would become a clinical mode of differentiation of great value were the biuret-reaction for albumin one of any delicacy.

[3] This seems well established, and yet not infrequently in urine-reports of cases it is noted that serum-globulin but no serum-albumin is present. In these instances the reacting body is probably nucleo-albumin, which, I believe, is nowhere noted as likely to be a confusing factor.

[1] This is important, in view of the frequent coëxistence of serum-albuminuria with a so-called mucinuria (nucleo-albuminuria).

[2] It is interesting to note that with approximately a 0.2 per cent. nucleo-albumin solution, containing NaCl, and a less amount of sodium acetate of a sp. gr. of 1017, the boiling-point has a slight precipitating effect. A further addition of a small amount of sodium acetate entirely prevented this precipitation in a second portion. Even without the presence of salts nucleo-albumin in small amount is apparently rendered more soluble by heat. A small amount shaken with distilled water, left, after a single filtration, a cloudy solution. Boiling at once cleared the latter.

[3] Save to trichloracetic acid.

Heat was most commonly applied in the manner detailed in the former paper,[1] by immersing the test-tube in hot water to a point beyond the upper line of fluid. In the trichloracetic-acid experiments there mentioned the bath was not always quite at 212° F., as so high a heat was not thought necessary. I was then unaware of the soluble effects of high heat upon traces of this mucoid body, accepting, as is commonly taught, that nucleo-albumin reacts to heat very much as does serum-albumin.[2] It evidently requires a temperature at or quite near 212° F., and prolonged—some minutes' contact—to insure a partial solution even with small percentages of nucleo-albumin. This is especially the case with trichloracetic acid. Nitric acid, and especially acidulated picric-acid solution, likewise retains a certain amount of the precipitant. With unacidulated picric acid the soluble effect of heat is more decided. The contact-coagulum obtained with it is dissipated to a greater degree by the aid of heat than with the others.

With potassium ferrocyanid and acetic acid, and especially with the metaphosphoric-acid test, however small the percentage of nucleo-albumin present, the soluble effect of heat is not evident. On the contrary, with the latter, heat tends to intensify the reaction obtained, so that it cannot in any manner be distinguished from that of serum-albumin. For this reason the clinical use of these reagents as precipitants of albumin must be regarded as absolutely untrustworthy, and should be unqualifiedly condemned.

It is important to study the effects of heat upon unprecipitated urine-solutions of nucleo-albumin. Heat, in any proportion of nucleo-albumin (in undiluted acid urine of ordinary gravity), upward of 0.15 per cent., is totally without precipitant effect. A few drops of dilute acetic or other acid which, added at ordinary temperature, cause a sharp cloud, are without precipitating effect upon the boiling urine. As cooling occurs, clouding results. When nucleo-albumin is present in proportion of from 0.3 to 0.4 per cent., whether ebullition was previously employed in obtaining the solution or not, clouding always resulted without the addition of acid on boiling. This behavior of the various strengths of nucleo-albumin, with urine as a menstruum, was invariable in a large number of experiments. It shows that when nucleo-albumin is present in amount approaching saturation, heat exerts a totally different effect from what it does on solutions of moderate strength. As these latter (upward of 0.15 per cent.) are, perhaps, even greater than can occur in any condition leading to the presence of nucleo-albumin in the urine, apart from a pronounced vesical catarrh, which cannot mislead, heat becomes the most important mode of differentiating a nucleo-albuminuria from a serumuria.

From these results it would follow that the only reliable test for serum-albumin, and that freest from liability to error, is the old-fashioned one by ebullition. But, apart from commonly recognized fallacies attending its employment, easily overcome, at least two important sources of error, strikingly developed in these experiments, are likely to arise with urine containing more than traces of nucleo-albumin. Treatises on urine-testing advise in the employment of the heat-test, the precaution that, should no reaction be apparent immediately on ebullition, the tube be inspected after standing and cooling, for cloud or precipitate, which, not being found to be due to acid urates, indicates albumin or an albumose. Now, as heat is without precipitating effect upon a naturally acid, nucleo-albuminous urine, to which no further addition of acid has been made in process of testing, precipitation of the mucoid body, under the circumstances, would not occur; but if an acid be added in the application of the test, clouding will surely result as the urine cools, unless the acid have been added in too small an amount to overcome the neutralizing effects of the urinary salts. As related, the experiments with mucoid nucleo-albumin, artificially added in moderate amounts to urine, indicate that high heat tends to dissipate the cloud produced by acids, although, after forming, it will not entirely remove all its traces.

A second source of error is that relating to the presence of an excess of earthy phosphates in a decidedly nucleo-albuminous urine. In the various experiments made with mucoid-fortified urines, specimens of the morning-urine were selected. On two occasions specimens of urine voided during the day were also employed as diluents, which, like the morning-specimens, were found not to react to trichloracetic acid within five minutes' contact in the cold. With these latter, and with one specimen of morning-urine, although all were acid, a cloud occurred in the fortified nucleo-albuminous specimens in the heated upper portion, which was not dissipated by the addition of a few drops of 25 per cent. acetic acid to 10 c.c. of the boiling urine. Control-specimens (containing no nucleo-albumin) were found to also cloud on heating, but were readily cleared by a small amount of acid. This indicates

[1] THE MEDICAL NEWS, May 5, 1894. This plan was pursued in order that mingling of reagent and urine is avoided when the contact-method is employed, as is so likely to occur by application of the flame directly to the tube.

[2] Treatises on urinalysis, save Neubauer and Vogel, state that urinary *mucin* is precipitable by heat. Reisser, quoted by Neubauer and Vogel, found that ebullition does not change urine rich in nucleo-albumin. (By rich, Reisser probably meant upward of 0.1 per cent., which is the greatest amount he at any time encountered.)

that the presence of nucleo-albumin in a urine rich in phosphates—a combination quite usual in the healthy in the day-urine, especially in that after food and exercise[1]—interferes with the solution of the precipitated phosphates by acetic acid, heat being coincidently applied to assist in the clearing of the mucoid reaction. This is an important point, ignorance of which has doubtless often given rise to error. It is of signal interest, as many urine-examinations, more especially those for life-insurance, are made of specimens voided under conditions giving rise to the presence of an excess of urinary mucin and of phosphates. But mucoid nucleo-albumin present in amount likely to originate error,[2] reacts to acetic or to citric acid, or to other of the vegetable acids, clouding more or less markedly at ordinary temperature, so that, by this means at least, the presence of excess is proved.[3] In testing for serum-albumin, should ebullition now not clear the acidulated urine, or at least not greatly diminish, the precipitate, the excess of earthy phosphates in a second portion of the urine should be removed in the usual manner, and heat later applied to the acid urine which has been first cleared by filtration.

It is of interest to here briefly detail the subsequent behavior of one of these albumin-free phosphatic urines to which about 0.2 per cent. of nucleo-albumin was first added, after the tendency to the marked precipitation of phosphates by heat was noted. The excess of the latter was precipitated by ammonia and removed by filtration. Later, the urine was rendered rather markedly acid and the nucleo-albumin thus thrown down removed. Repeated filtration gave a very clear solution, of a specific gravity of 1025. This, at ordinary temperature, still gave a cloud to acetic acid, but, however, tended to become quite clear on ebullition. Specimens gave a contact-cloud with nitric acid and a marked contact-ring with Millard's reagent; with trichloracetic acid the reaction was very decided; with metaphosphoric acid it appeared within a minute and was marked; with unacidulated picric-acid solution no immediate reaction occurred, but at the end of a minute a contact-haze was discernible, not increasing to an outspoken ring after standing for some time. With citrated picric-acid solution a fine, sharp ring was instantaneous. With all of these reagents, save metaphosphoric acid, the extent of reaction was diminished by the application of decided heat, although with trichloracetic acid this reduction in amount of response was less manifest than with the others. The reaction of this specimen to the various tests was most instructive, as it contained a very small amount of the mucoid body, calculated by similar response of other specimens, to which measured amounts were added, to be 0.02 per cent. or less. This specimen was of especial interest for study because of correspondence in point of response to several specimens of day-urine from healthy subjects I had recently examined.[1] These gave a pronounced acetic and citric acid mucoid cloud, and responded quite markedly to the delicate tests for albumin, gave a narrow cloud at or near the meeting-line with HNO_3 by the contact-method, but did not react to heat alone.

So far as I am aware, the valuable aid heat may render toward promoting a response, in the application of tests used by the contact-method, for the detection of serum-albumin is nowhere mentioned. As apparently heat also becomes a factor of importance in the separation of the so-called mucin-reaction from that due to serum-albumin its employment is most essential when there exists the slightest doubt as to differentiation. I have frequently been able to bring out a serum-albumin reaction with HNO_3 used by the contact-method, by placing the test-tube in hot water for some moments, which response otherwise was most tardy in appearing, if eventually noted at all. The use of heat in this way, rather than the application of the naked flame to the tube, is essential, in order that actual ebullition in the tube be not permitted, otherwise disturbance of the relative relation of reagent and urine would interfere with the occurrence of a sharp contact-ring.[2]

Although I think it may be confidently asserted that a reaction so induced, as with one also apparent without heat, if decidedly sharpened by suspension of the tube in the water-bath, is due to serum-albumin (or serum-globulin), yet, on the other hand, as heat will not entirely remove a mucoid response after it has once appeared, there can be no great certainty as to the absence of recognizable traces of serum-

[1] Exercise and the process of digestion tend to increase the amount of mucoid substance in the urine. From the merest trace, undetectable in the morning-urine, it may rise in amount sufficient to cause a reaction to the contact nitric-acid test. I have noted this in the urines of several healthy male adults since examining into this point.

[2] In the phosphatic urine mentioned, an amount of nucleo-albumin was added approximating 0.1 per cent.

[3] I have elsewhere dwelt upon the precautions necessary to insure a mucoid response with acetic acid, in urines with varying amounts of salts in which recognizable traces of mucoid nucleo-albumin are naturally present. The precipitating effect of acetic acid on urates must be borne in mind. The latter are differentiated by the fact that their precipitate is colored and has a characteristic microscopic appearance. They may be also recognized by the fact that a gentle heat, short of that necessary to diminish a mucinous reaction, clears the urate-precipitate. Dilution of the urine, before the addition of acetic acid, will also tend to obviate a urate-precipitate and to increase a mucoid one.

[1] Tested in all points similarly to the reinforced mucoid urine save as regards the dissolving effects of strong heat.

[2] By this method acid urates and a mucoid reaction are both prevented.

albumin in the presence of the nucleoid body. If smaller amounts of albumin are looked for, the same must be said of the use of the delicate tests, although it would appear that if these are applied[1] with reagent and urine at the boiling-point—a method, of course, impracticable in ordinary routine urine-testing—no mucoid reaction will occur so long as these are maintained at a high heat. Of the many delicate reagents, unacidulated solution of picric acid has the least reacting power to traces of nucleo-albumin. Decided preference should, therefore, be shown it over all of the others, if the employment of a refined test seems essential. Citrated or otherwise acidulated picric acid should never be employed. Especially should it be avoided in the quantitative estimation of albumin by Esbach's method.

A serious fallacy attending the influence of heat upon the trichloracetic-acid reaction in the experiments with nucleo-albumin should be here mentioned. It tends to further illustrate forcibly the impracticable, extraordinary delicacy of this reagent as a precipitant of albumin, and its inutility for clinical purposes. In the course of the investigation a control-specimen of acid urine previously unfiltered, of that portion used for experiment which gave no reaction at ordinary temperature with trichloracetic acid until some minutes' contact,[2] was thoroughly boiled and filtered while hot. This was done with the object of obtaining a control-solution in all particulars similar (save as regards the presence of nucleo-albumin) to the fortified nucleo-albumin specimen. To my surprise, it was found that this boiled and subsequently but once filtered control-specimen reacted more markedly to trichloracetic acid than did the unboiled, unfiltered, original specimen. I then found that both cold and hot distilled water ran once or twice through specimens of all of the various makes of white filter-paper in my laboratory, gave either instantly or after the lapse of a few moments the contact white ring with trichloracetic acid.

I then procured specimens of the very best grades of fine filter-paper other than those which I had. It was thought that some of these would certainly not so react. Bearing as this matter does upon the subject of fallacies attending albumin-testing, the result is worth mention. The specimens, four in number, obtained from Messrs. Queen & Co., were marked *Swedish; French;* No. 390 and No. 589. They were of equal size, with a capacity of about 20 c.c. Two of each variety were used, one for hot distilled water, the other for distilled water at room-temperature; from 5 to 7 c.c. of each were passed through the filters in the usual manner several times. No pressure was exerted to force the water through. The distilled water in each instance was free from trace of cloud after filtration as before.[1] A slight contact white ring characteristic of traces of albumin occurred instantly with all of the specimens of distilled water filtered while hot, and tested in all save with the specimen of French paper while still warm. The albumin-reaction was quite decided with paper No. 390. With the cold, filtered, distilled water the reaction was instantaneous only in two, with but one filtration; in the others it appeared in from one-half to one minute. In all, the reaction became very much more decided on standing a short time; but although the use of the water-bath had no diminishing effect, heat did not seem to noticeably increase the reaction, as is the case with serum-albumin. A fine cloudy haze throughout the body of the tube was, however, developed by heat, indicating a precipitating effect of the trichloracetic acid, aided by heat, on the minute traces of albumin present. Experiments of various other sorts were tried with these solutions and trichloracetic acid, which, though interesting, cannot be detailed here. Millard's reagent, plain or citrated picric acid solution, and the reagents of the tartrated mercuric chlorid were found to give no indication of reaction, even after standing in contact for some time.

Millard, a number of years ago, called attention to the fact that the coarse French gray filter-paper, which contains a great deal of cellulose or vegetable albumin, would yield sufficient to react to his reagent. This I long ago confirmed, finding that several of the delicate tests reacted to distilled water passed through the gray paper.[2] Trials made at that time with the white Swedish, French, and German makes, and the same tests, were resultless, so that it had not been suspected that a fallacy from this source, with a good quality of filter, existed.

To remove the traces of nucleo-albumin from suspension, it was necessary to repeatedly filter its menstruum; from three to six papers were always employed, all of which probably contributed their quota of albumin to the filtrate, which would increase the amount of albumin recognizable by trichloracetic acid uninfluenced by heat.

Trichloracetic acid as a reagent for the detection of albumin is of such exceeding delicacy that, though evidently of little practical utility for clinical purposes, it apparently may be utilized to demonstrate, without need for concentration of urine, the existence of a constant normal albumin-trace. A phys-

[1] Excepting metaphosphoric acid.

[2] And then but a very fine white ring, showing rather less than the ordinary normal traces of albumin.

[1] For the sake of exactness, a control unfiltered specimen of distilled water was also tested with trichloracetic acid. As might be expected, it gave no reaction.

[2] Trichloracetic acid had not then been proposed as a test for albumin, and thus was not used.

iologic albuminuria asserted to exist by Leube,[1] Fürbringer,[2] Kleudgen,[3] Senator,[4] Chateaubourg,[5] Capitan,[6] Posner,[7] and others, has been as strongly questioned by v. Noorden,[8] Millard,[9] Lecorché and Talamon,[10] and many others. The confines of this paper do not admit of even a cursory survey of existing data for or against this important question; the subject is one that requires much space for its consideration and will receive attention subsequently. Suffice it here to state that much of the work was done with concentrated urines, and the question of the frequent presence normally of extra-renal mucoid nucleo-albumin was often altogether ignored by those favoring the existence of a physiologic albuminuria. Leaving out of consideration the frequent high percentage of marked albuminurics among the apparently healthy, to be accounted for as a result of digestive derangements, circulatory weakness and the like, not omitting a latent nephritis, the fact that many of the experimenters in the field of physiologic albuminuria, who confidently hold to the existence of a normal minute albuminuria, fail to consider the presence of nucleo-albumin as a complicating factor, has thrown much doubt upon the results arrived at. Still regarding albuminous bodies other than serum-albumin which may be present in the urine and are calculated to mislead, the experiments of many observers[11] have at least shown that at times no reaction may be obtained with such delicate reagents as Tanret's and Millard's; and Leube failed to get an albumin-response in some entirely healthy urines which had been subjected to much concentration. If the experiments which I have detailed with the reactions of mucoid nucleo-albumin to certain of the tests for albumin represent those obtainable when this body is present in traces in healthy urine, as I think there can be little doubt, then it can be proved that Posner is right in asserting that all urines contain demonstrable traces of albumin, and that a physiologic constant minute albuminuria exists, an albuminuria which apparently is not due entirely to the presence of a mucoid body, although it may perhaps still be accidental or contingent, and not arising from the blood-serum of the renal vessels.

In the paper on fallacies attending the employment of delicate tests for albumin it was stated that in a series of 105 urines from presumably healthy male adults, 102 gave at ordinary temperatures a contact-reaction characteristic of albumin within five minutes, and that the remaining three also readily reacted within this time on the employment of heat, showing a contact white ring of one or more lines in depth. The reaction in the 102, further tested by heat, was apparently increased.[1]

Since the series of 105 consecutive urine-examinations, in all of which the specimens reacted, as stated, to trichloracetic acid, I have examined a series of 35 morning-specimens from presumably healthy males in the prime of life, none of whom contributed any of the preceding specimens examined.[2] With the exception of but 2, applying the technique detailed in the other paper, without the aid of heat, a reaction was noted in all[3] within five minutes. In nearly all of these it was quite marked in this time. In the remaining 2 a contact-reaction was noted within ten minutes. In 13 the reaction was instantaneous, but in 2 only was it immediately very decided, and these were found to be examples of markedly albuminous urine reacting to coarse tests. In 6 only of the 33 could the reaction be termed slight at five minutes' contact.

The effect of heat upon all of the specimens which had reacted before its application was carefully studied, and was noted to vary somewhat with different specimens. In some it very markedly and promptly intensified the sharpness and depth of the contact-ring, which had not been very decided before the use of the bath. With 4 the effect of heat was negative, viewing the tube after some minutes' exposure to the bath. The contact-cloud remained as before, deepening, however, quite markedly, as was the case with all, after cooling and standing. It was certain that in all heat developed earlier a reaction which, without heat, was several minutes in becoming well-defined. In one of the 2 in which no response occurred in the cold until after five

1 Virchow's Archiv, 1878, lxxii, 145.
2 Zeitschr. f. klin. Med., 1880, i, 346.
3 Arch. f. Psych., 1881, xi, 5, 478.
4 Die Albuminurie. Berlin, 1882.
5 Thèse de Paris sur l'Albuminurie Physiologique. Paris, 1883.
6 Thèse de Paris sur Albuminuries Transitoires. Paris, 1883.
7 Berliner klin. Woch., 1885, xxii, 654; Virchow's Arch., 1886, civ, 497.
8 Deutsches Arch. f. klin. Med., 1886, xxxviii.
9 Bright's Disease. New York, 1892.
10 Traite de l'Albuminurie et du Mal. de Bright. Paris, 1888.
11 Lecorché, Talamon, Millard, and others.

1 As the influence of heat upon a slight mucoid reaction was not then understood, less attention was paid to long contact in hot water; nor was the latter always maintained at the boiling-point, it being more especially desired to differentiate as to urates rather than as to varieties of albumin. The nucleo-albumin experiments show that unless the heat be decided, and the amount of nucleo-albumin present small, the soluble effect of the former is not apparent. In fact, although lessening of reaction with heat was quite constant with traces of nucleo-albumin, it was not always so to the same degree. It was common, too, for a mucoid response to become again decided as the temperature of the water fell from boiling-point to a degree of heat still decided to the finger.

2 Thirty-one of these 35 were collected for me by my friend Dr G. M. Gould, to whom I am under warm obligations for them.

3 In the specimens of the 105 urines less than 10 of the number had been filtered before testing. In none of the present 35 was filtration practised. The portion to be tested was always taken from the upper stratum, after subsidence of any mucous cloud in suspension.

minutes' contact, it was noted that heat within a moment or so produced a contact-cloud of a depth of three or four lines, which cloud supplanted the slight, although more sharply defined, white ring evident without heat. The urine above in the tube also subjected to the bath became hazy. On inspecting the cooled tube subsequent to the use of heat, after it had been standing for some time, a sharply defined contact-ring was again noted of a much greater extent than at first, with a deep cloud above of some lines in depth.

One fact seems to me certain, developed by the examination of a series of nearly 150 specimens of urine from young male adults, the majority of whom are apparently in robust health, and none of whom had been regarded as renal suspects, that, excluding the 22 marked albuminurics, whatever the nature of the albuminous body reacting, a response may be obtained in any specimens of all urines of the healthy by the intelligent employment of trichloracetic acid. This response may not occur instantly, but usually develops within a few moments, and may be brought out in any instance by the use of heat in the manner described.

SYPHILIS OF THE NERVOUS SYSTEM.

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VIRCHOW has said that the cerebral manifestations of syphilis are as manifold as the cutaneous, and that the spinal manifestations are as manifold as the cerebral.

The early recognition of the essential nature of these various manifestations is of the utmost importance, for nowhere else in the whole range of therapeutics does prompt and vigorous treatment produce such wonderful effects as in syphilis of the nervous system.

The lesions produced by syphilis are of three kinds: first, the gumma; second, syphilitic endarteritis; third, the diffuse syphilosis of Heubner. In addition to the symptoms produced by these lesions, which act mainly in a mechanical way, the toxin of syphilis, under conditions which we do not at all understand, may directly attack the protoplasm of the nerve-cells or the axis-cylinders, which are composed of the same substance, as in paretic dementia and locomotor ataxia. This view finds support in the fact that central paralyses and psychoses may be caused by the toxins of other infectious diseases. Such occurrences have been reported as sequelæ of influenza, typhoid fever, measles, pneumonia, rheumatism, diphtheria, scarlet fever, and erysipelas. In these there can be no question of an extra-neural lesion, as in most cases of nervous syphilis; the toxins must have acted directly upon the nervous protoplasm, and any other changes found must have been secondary or merely coincident. We know that the toxins of various germs undergo great variation after prolonged cultivation or passage through the bodies of various animals. To mention a single instance of this, the virus of hydrophobia increases in virulence when passed through guinea-pigs, but diminishes in virulence when passed through monkeys. Why may we not reasonably suppose that the virus of syphilis, after prolonged cultivation in the body, may undergo such changes that, instead of producing its customary lesions, it acquires the power of directly attacking the nervous protoplasm? That such changes occur in the toxins of other infectious diseases is plainly shown by the fact that the central paralyses mentioned do not occur until long after the primary symptoms of infection have disappeared. In typhoid fever, for instance, these paralyses occur during convalescence, some eight or ten weeks after the primary infection.

When and by whom was the canon of syphilitic lesions declared closed, and who can say, "Syphilis can only produce a certain number of lesions of the nervous system; I deny that any other changes are specific in character?" To claim, as is so often done, that syphilis merely predisposes to paretic dementia and locomotor ataxia is, as Strümpell has well said, simply a meaningless expression. It is as if someone should say, "Typhoid fever can only produce one characteristic lesion, which consists in certain changes in Peyer's patches. If central paralyses occur during its course, I deny that these are directly due to the typhoid fever, though it may act as a predisposing cause"—and so on through the list of infectious diseases. The fallacy of such reasoning is apparent.

Syphilis during its various grades of culture in the human body shows a preference first for one and then for another of the tissues—attacks now the skin, now the mucous membranes, now the bones, then the meninges of the brain or cord, or the walls of the bloodvessels, etc. It is only necessary to add another category of specific lesions, viz., those cases in which it attacks the nervous tissue proper.

We might class the various forms of nervous syphilis under the following heads:

1. Gummatous.
2. Vascular.
3. Interstitial.
4. Parenchymatous.

Gummous syphilis produces, of course, merely the symptoms of tumor. If the gumma be in the motor area, there will be epileptiform attacks or paralysis, according to the degree of pressure; if it press upon the cord, a compression-myelitis will

occur; if it impinge upon a nerve, a neuritis will result, and so on.

In vascular syphilis the cerebral symptoms are those of apoplexy, from rupture of a bloodvessel, and thrombosis with its attendant cerebral softening, which is brought about by the narrowing of the lumen of the vessels and roughening of the internal coat from endarteritis. Cases of acute transverse myelitis, following syphilis, probably arise in the same way; while in chronic myelitis there is a slow cutting off of the blood-supply, but no formation of a thrombus.

The interstitial form is attended by an increase in the connective tissue, its organization into bands of sclerotic tissue, the occurrence of a syphilitic exudate, and so on. This gives rise to meningitis, which may extend into the cord and produce a meningo-myelitis, or it may produce an interstitial encephalitis.

Under the head of parenchymatous syphilis are to be included the syphilitic psychoses, syphilitic neurasthenia, paretic dementia, locomotor ataxia, progressive bulbar paralysis, progressive muscular atrophy, and amyotrophic lateral sclerosis.

The psychoses are placed under this head for the reason that similar affections are often produced by the toxins of other infectious diseases, notably by that of influenza, which must act directly upon the nerve-substance; the symptoms are too widespread to be due to gumma; the complete recovery of many of these cases would seem to shut out interstitial and vascular changes, for these leave permanent results.

The propriety of placing paretic dementia under this head will, no doubt, be questioned, for it has been much discussed whether the primary changes in this disease are interstitial or parenchymatous. The weight of authority seems to be in favor of the latter view. Hirt states that "this view, according to which the atrophy is the primary process, is in all probability correct, though it is still combated by many authorities (Mendel), who look upon the death of the nerve-fibers as the secondary, upon the increase of the connective tissue, the thickening of the vessel-walls, and the appearance of spider-cells as the primary process."

Ziegler says, in his *Lehrbuch der allgemeinen Pathologische Anatomie und Pathogenese:* "The disturbed nutrition and the degeneration of the ganglion-cells and nerve-fibers are the important matters; while the inflammatory infiltration and the increase in the connective tissue have no real significance as far as the symptoms of the disease are concerned."

And Strümpell remarks: "We ourselves, with Tuczek, Wernicke, and others, are much more inclined to the theory that we have to do mainly with a primary process of degenerative atrophy of the nerve-fibers and nerve-cells, to which the increase of the connective tissue is only a secondary addition."

Finally, the influence of chronic alcoholism in the production of this disease, upon which Magnan lays such stress, would seem to sustain this view, as alcohol is a parenchymatous poison.

Precisely the same discussion has been waged concerning the primary lesion in locomotor ataxia, some holding that the disease first causes an atrophy of the axis-cylinders of the affected nerve-fibers, which is followed by a sclerotic process, while others maintain the opposite view. The trend of opinion at present seems to be toward the first view.

The same considerations apply, of course, to the question of the intimate nature of syphilitic neuritis.

The question of diagnosis brings up Wood's famous therapeutic test, viz.: "The vast majority of persons who are free from syphilitic affection cannot take doses of over ten grains of potassium iodid three times a day without the production of iodism, except as the result of the habitual use of the remedy."

The active discussion which the announcement of this rule brought forth served to arouse the attention of the entire profession in America upon this point. So many cases of tolerance of iodids in large doses in which there was no suspicion of syphilis were found, on the one hand, and so many undoubted syphilitics who could not tolerate thirty grains *per diem*, on the other hand, that I believe that I am stating the general opinion on the subject in saying that the word "vast" should be stricken out.

The prognosis is determined by two considerations—the length of time the disease has lasted and the nature of the lesion. Many physicians have the idea that because the disease is syphilitic the prognosis is invariably good; but one should consider how much of the new tissue is syphilitic, and therefore removable by treatment, and how much is extra-syphilitic and permanent. Specific remedies will cause a gumma to disappear, but they cannot dissipate the connective-tissue growth which the presence of the gumma has called forth. Victor Horsley has gone so far as to advise the surgical removal of well localizable gummata.

The prognosis seems to be best in the gummatous form of nervous syphilis, for the reason probably that this soonest of all produces marked symptoms which call attention to its presence. If a man suddenly develops epileptiform attacks or paralysis, he will at once consult a physician, and if the cause is recognized the prospect of permanent and entire relief is good. But a patient might suffer for months with the much more vague symptoms of syphilitic endarteritis without consulting a physician until the

occurrence of thrombosis would place him beyond all hope.

The prognosis is next best in parenchymatous syphilis. Out of twenty-three cases of syphilitic psychoses collected by Wood in his monograph there were twenty-one recoveries, one apparent recovery, and one relapse, which resisted all specific treatment. Now, it is entirely out of the question that these psychoses should have been produced by gummata, or even by multiple gummata, for the symptoms were too widespread, involving the entire cortex. They must, then, according to prevailing ideas concerning syphilitic lesions, have been due to interstitial or arterial changes. But if such changes had progressed so far as to produce marked psychoses, could these patients have made a complete recovery? Is there any evidence that specific treatment can restore the lumen of a vessel obliterated by syphilitic endarteritis or remove interstitial connective tissue? Is the sclerotic tissue in these cases in any sense specific, does it differ in anywise from other sclerotic tissue, is it any more susceptible of removal by treatment when it has once formed? Is it not precisely in those cases in which the lesion is interstitial or arterial that treatment does least good or entirely fails?

If we admit, on the other hand, that there is a parenchymatous form of nervous syphilis; that the cells of the cortex have a wonderful power of resistance to poisons, as is shown in the prolonged abuse of alcohol and opium; that their action may be perverted by various poisons for years, and yet when the poison is removed they will entirely recover their functions; that when they are overcome by the poison and die, their place is filled in by connective tissue—admitting these things, we have, it seems to me, a satisfactory explanation of the occurrence of these psychoses and recovery from them.

Even in syphilitic tabes, according to Erb's statistics, the prognosis is fairly good, if the secondary sclerotic process has not advanced too far.

Gowers has announced an exceedingly important fact bearing upon the treatment of syphilis, viz.: that the germs rapidly acquire a tolerance of the iodid, which they as rapidly lose. Hence the best results are gotten by administering iodid for a period of six weeks, and discontinuing it for a like period. The continuous use of the iodid defeats its object.

Certainly there are few American physicians who will agree with Gowers when he says: "I believe that this dose of iodid (grs. xv t. i d.) is adequate, and will do all that can be done in the course of four to six weeks. It is better not to give larger doses, because coagulation of the blood plays a part in the mechanism of destruction, and large doses of the iodid, as the history of aneurism has taught us, increase the tendency of the blood to clot." The use of massive doses of the iodid in these conditions originated in the United States, and certainly every one has seen cases in which fifteen grains three times a day would not affect the condition, but which improved rapidly under several hundred grains *per diem*.

In choosing between the iodid and mercury, the greater rapidity with which the latter acts should be borne in mind. Nervous syphilis is so treacherous a disease, the lightning is so apt to strike one out of a clear sky, that it is certainly wise to lose no time, but to begin treatment with a thorough course of mercurial inunction. This method of administering mercury is far preferable to any other in this class of cases.

The importance of varying and individualizing the treatment should be remembered. When the case has come to a standstill under the ordinary remedies, undoubted good is done by the vegetable alteratives.

A very important matter is that of prophylaxis. In how far is nervous syphilis preventable? This is classed among the tertiary symptoms. The following are Fournier's latest statistics upon this subject. Among 1703 cases tertiary symptoms appeared in the following numbers, their previous treatment being known:

	Cases.
No treatment	217
Short treatment (under one year)	1162
Moderate treatment (from one to two years)	265
Long treatment (over two years)	53
Treatment exceeding three years	6
	1703

"We find, then," says Fournier, "that among 1703 cases there were 59 in which tertiary symptoms appeared after, and in spite of a vigorous and, we may believe, sufficient treatment; while there were 1644 cases after an abbreviated, insufficient, or expectant treatment."

Again, in 100 cases of cerebral syphilis, Fournier found 5 which had received vigorous and prolonged treatment with mercury; in 6 cases the treatment was of a moderate but doubtless insufficient duration; 10 patients were only treated from 7 to 18 months, and 70 from 1 to 6 months; in 4 there had been no treatment; and 5 had taken nothing but the iodid.

Fournier draws the following conclusions from these facts: "Out of 100 patients, 5 had received a thorough and prolonged treatment, as against 95 who had received some other treatment, or none at all. So, then, syphilis of the brain is nineteen times rarer after prolonged treatment."

As to what proportion of cases present nervous syphilis, Fournier states that among 3429 cases, 1085 were instances of syphilis of the nervous system.

In discussing these statistics before the Wiener Medizinisches Doktoren-Collegium, November 6, 1893, Dr. Finger remarked that at the Congress in 1889, when Fournier presented these statistics, other authors, Mauriac, Haslund, Leloir, reported very similar results. As to syphilis of the nervous system, it should not be overlooked that Fournier considers tabes, progressive paralysis, and the muscular atrophies as syphilitic.

The object of therapy in syphilis is to prevent the appearance of tertiary phenomena. It is well known that the frequency of the tertiary symptoms is in inverse ratio to the lightness of the secondary stage, and this is for the reason that we adapt the symptomatic treatment to the light, secondary stage. The symptomatic treatment is a defensive procedure; it deals with the present symptoms. A portion of the virus remains behind, and, thanks to the treatment, has time to increase and make a new attack. This again is dealt with defensively, and so the scene repeats itself over and over. It is much more logical constantly to attack the virus. But as the mercury accumulates in the system and the latter becomes accustomed to the poison, there should be intermissions in the treatment. Finger had used the prolonged treatment for ten years, and had very seldom seen tertiary symptoms in his patients or hereditary syphilis in their children (*Deutsche med. Zeit.*, 1893, No. 95).

As tertiary symptoms have occurred as long as fifty-five years after primary infection, one had best give these patients a life-sentence, and tell them that they must keep up treatment the rest of their lives. Let them take six weeks' courses four times a year. The physician should lose no opportunity of dangling Damocles' sword of nervous syphilis over the heads of these patients. It is amazing what perfect indifference many of them show and how difficult it is to make them keep up treatment.

In conclusion, I wish to report briefly some of the cases of syphilis of the nervous system which have come under my notice and which present points of interest:

CASE I.—This patient had progressive aphasia from a syphilitic lesion. There were no other symptoms. He lost his parts of speech in regular order, following exactly Ribot's law; first proper names went, then adjectives, and so on. Finally his whole vocabulary was reduced to interjections, expletives and oaths. He recovered his parts of speech in the inverse order—in the order in which evolutionists say that mankind acquired them. This case serves to show what a deep, fundamental need of our nature profanity supplies. The lesion was evidently a gumma exactly limited to the speech-center.

CASE II.—I was called in consultation to see a policeman. He had complained of headache for a day or two, and then in a few hours became comatose. When I saw him his breathing was slow and stertorous, pupils uneven, pulse full and labored, and there was left hemiplegia. I felt strongly inclined to bleed him. A history of syphilis being obtained, he was ordered mercurial inunctions, to be followed by increasing doses of the iodid. In six weeks there was no trace of the hemiplegia left. This was probably one of those singular cases of sudden congestion about a gumma, like the apoplectiform attacks of multiple sclerosis.

This man now presents uneven pupils, hesitancy in his speech, failure of memory, and fibrillary tremor of the lips; but in spite of all I can do, it is impossible to get him to keep up his treatment. When he has his next attack I shall certainly bleed him.

CASE III.—G. X., a Portuguese sailor, was an inmate of the Louisville City Hospital for about a year and a half. No history could be gotten. He had all the signs of cerebellar involvement, strabismus, diplopia, vomiting, and titubation. His gait was like that of a drunken sailor on shore after a long sea-voyage. He was given the benefit of the the doubt and placed upon specific treatment, taking at one time as high as 200 grs. of the iodid at a dose. This produced rapid improvement up to a certain point, but beyond this it was impossible to benefit him by any amount of treatment. Probably what had happened was that the syphilitic portion of the cerebellar gumma had been absorbed, leaving behind a considerable mass of scar-tissue.

CASE IV.—J. H. thirty-four years of age, suddenly developed violent epileptiform attacks. They came quite close together, and in the intervals between them he complained greatly of headache and insomnia and was very confused mentally. I saw him about an hour after the third attack, which came within a week of the first one. He was sitting stupidly upon the side of the bed, unable to recognize anyone. In a case of this sort it is hardly necessary to ask a single question. He was put upon treatment at once and made a complete recovery, never having another attack. This case shows how favorable the prognosis of gummatous syphilis is *sometimes*.

CASE V.—L. F., a lawyer, aged forty-five, noticed one day that his left foot dragged a little and he had difficulty in raising the toe of his shoe from the ground. By the next morning the weakness had extended to the arm. I saw him the afternoon of the same day, when the left side of his face was somewhat involved. On inquiry he admitted that he had had a sore on his penis some twenty years before, which had healed up after six weeks of treatment. Evidently there was a gumma situated near the median line on the right side and spreading down over the motor tract of the right hemishpere. Remembering my experience with Case IV, I gave a favorable prognosis. The patient improved so rapidly under mercurial inunctions that he was able to leave for Atlanta in a week, where he died about two months afterward. Whether the treatment was not properly carried out, or whether it was one of those singular cases which prove utterly rebellious to treatment, I am unable to say.

CASE VI.—J. T. entered the Louisville City

Hospital on Wednesday morning in a state of great mental confusion, and unable to give any account of himself. That night he became maniacal and had to be strapped in bed. I saw him Thursday morning. He was comatose and had left hemiplegia. There were characteristic scars over the tibia. On making firm pressure over the left tibia the patient drew up his right leg and made a defensive movement like a Goltz frog. That night he was so wild that he could hardly be kept in bed. He died on the next morning. A post-mortem was held that afternoon. We found softening of all that part of the right hemisphere supplied by the Sylvian artery, which might well be called the motor artery. The arteries at the base were grayish in color, resistant to touch, and patulous. The caliber of the Sylvian artery was reduced to the size of a knitting-needle, its inner coat was roughened, and it was completely obstructed by a thrombus.

CASE VII.—P. S., foreman of a bridge-company, forty years of age, entered my office one day with marked ptosis. I asked him at once, "How long is it since you have had syphilis?" "Twenty years," he responded, greatly astounded at the omniscience of the medical profession. Ptosis in an adult is a symptom which points very strongly to nervous syphilis. The patient dated his present trouble from a blow received upon the head about three months before. It has been very much debated whether injuries to the head have any localizing influence in such cases. He had had very little treatment at the time of the initial lesion; there were no secondary manifestations. Recovery followed.

CASE VIII.—Mrs. S., a widow, twenty-nine years old, had suffered from constant headache for nine weeks, for which she had received hypodermatic injections of morphin and ten-grain doses of potassium bromid, that panacea for all diseases of the nervous system. When I saw her she had reached that stage when she could neither sleep nor remain awake. About three weeks before, she had had an attack in which she fell to the floor and lay unconscious for perhaps twenty minutes, when her daughter came in and helped her to bed. She knew nothing more then for twelve hours. There were nocturnal exacerbations of the headache. Examination revealed the presence of a tumor in the forehead about the size of half a pea, which was exquisitely sensitive to pressure. She had lost thirty or forty pounds in weight and could eat nothing. This case would probably have ended fatally in a short time if its nature had not been recognized. After forty-eight hours of vigorous treatment the headache showed signs of yielding, and has since entirely disappeared. This was a genuine case of "syphilis insontium," as the woman was so ignorant that she did not know the meaning of the word syphilis. Her husband had died about three years before of a "relapse of the measles," at Hot Springs.

I debated some time whether or not I should inform her of the true nature of her headache, and finally concluded that it was best to do so, as she might fall into ignorant hands again. She gradually recalled certain significant facts. She had been treated once for a mysterious uterine disease; her husband held long secret confabs with the doctor in the hall. There had been a sore at the corner of her husband's mouth, which the doctor had warned her she must be careful not to touch in case she had any sores upon her hands. She has two children with Hutchinsonian teeth. Subsequently she developed syphilitic iritis, which yielded promptly to treatment.

CASE IX.—Dr. Ap. Morgan Vance referred to me a little girl, two years old. The mother gave the history of having had two children stillborn at full term, and one miscarriage. The child had marked spastic paraplegia, with greatly exaggerated tendon-reflexes, and Hutchinsonian teeth, and was very backward in mental development. She could not crawl upon the floor, and her vocabulary consisted of three or four words. The treatment consisted in mercurial inunction, prolonged warm baths, and galvanism to the spine. In eight weeks there was marked improvement in the symptoms, the mental change being especially noticeable. She has continued to improve steadily since then, and is learning to walk and to talk. Another similar case sent me by Dr. Vance has shown improvement under the same treatment.

CASE X.—J. K., forty-seven years of age, gave the history of a chancre some twenty years ago, for which he took treatment until the sore healed. No secondary symptoms followed. About three years ago he began to notice that it was difficult for him to walk at night, so that he stopped going out after dark. I saw him several months after this, when he presented the following symptoms:

Argyll-Robertson pupils, girdle-sensation, vesical disturbance, swaying with the eyes closed, anesthesia of the lower extremities to a slight degree, ataxic gait, and exaggerated knee-jerk. This case corresponds to Erb's type of spinal syphilis. No improvement followed under potassium iodid carried up to 600 grs. per diem, and there were no symptoms of iodism. There was some improvement under mercurial inunction.

CASE XI.—I was called to the City Hospital one night to see a woman who had suddenly developed acute mania. She was strapped in bed with her hands in the muff, and was filled with a burning desire to cut somebody's heart out. She quieted down under a large dose of chloral. The diagnosis of cerebral syphilis was easily made, as she was an "old rounder" who had formerly been treated for a chancre. Vigorous specific treatment was at once instituted, and she made a prompt and complete recovery. No other cause but syphilis could be discovered for the attack. It seems hardly possible that it could have been due to grumma, to interstitial syphilis, or to syphilitic arteritis. Yet the ready response to treatment showed that it was specific in nature.

CASE XII.—One of the internes at the City Hospital called my attention to a man who was sitting half asleep upon the edge of his cot. When aroused he stated that he could not remain awake for five minutes at a time, neither could he sleep soundly. At night he was in a condition bordering upon mild delirium, and showed a disposition to get out of

bed and wander about the ward. This state is well nigh pathognonomic of cerebral syphilis, which may produce the condition of extreme sleepiness or extreme wakefulness. The patient made a speedy recovery.

CASE XIII.—J. D., a laborer in the glassworks in New Albany, Ind., developed symptoms of locomotor ataxia about three years after the initial lesion. The case progressed so rapidly that when I saw him, twelve months afterward, he was far along in the third stage, with complete optic atrophy, loss of control of the sphincters, etc. He died about two months afterward. The nature of his disease had not been recognized, and he had received no specific treatment. He stated that the room in which he worked at the glassworks was so full of gas that it was no unusual thing for a man to be overcome by it and fall unconscious. What effect this may have had in the development of his spinal lesion it is impossible to say.

CASE XIV.—W. T., fifty-one years of age, was entered in the case-book at the City Hospital as a case of locomotor ataxia, the simple definition for this disease in vogue at that institution at that time being, "a disease of the spinal cord, causing disturbance of gait." The man really had paretic dementia of syphilitic origin, with descending degeneration of the crossed pyramidal tracts, producing spastic paraplegia.

There were delusions of grandeur. He promised a horse and buggy to each of the internes. Besides this, he had uneven pupils, fibrillary tremor of the lips, and other signs of paretic dementia. He left the City Hospital and wandered about the streets for several days, sleeping in outhouses. The last time I saw him he was sitting upon the curbstone, with his feet in the gutter. He hailed me cheerfully, and said, "I'm goin' to send you that horse and buggy next week, shore."

215 W. CHESTNUT STREET.

A TREATMENT OF TYPHOID FEVER.[1]

BY SAMUEL A. FISK, A.M., M.D.,
OF DENVER, COLORADO.

IN these days, when the Brand treatment of typhoid fever is the only one receiving any consideration, and the discussion in medical circles, with reference to the treatment of this disease, is confined to a consideration of hydrotherapy, it seems a little presumptuous for one to come forward with another presentation of the subject. My excuse must be that an observation of the results obtained by another method of procedure, extending over a number of years past, has led me to the conclusion that there is in this method something that is, at least, worthy of consideration. I am duly sensible that the presentation that I can give of my subject, at this time, is not based upon such a series of cases so scientifically conducted as to carry the fullest conviction, and my regret is that the hospital-services of a somewhat new community do not afford the same opportunities for careful and scientific investigation as exist in older and more settled centers. That my opportunities for observation have not been altogether limited will be illustrated by a study of the mortality-tables of Denver and of other cities.

In his recent report on typhoid fever, Professor Osler states that for the years from 1888 to 1892, inclusive, Baltimore gives a yearly average of 229 fatal cases; Boston a yearly average of 160.4 fatal cases; New York a yearly average of 379.4 fatal cases; and Philadelphia a yearly average of 661.8, while Denver for the same length of time gives a yearly average of 149.6.

My observation of typhoid fever, both in private practice and in hospital-service, during these years has brought me to the belief that the line of treatment which I, in common with a few others in Denver, have followed was productive not only of a small rate of mortality, the exact ratio of which I unfortunately cannot give, but also of a mild run of the fever, a minimum of complications, and a very decided diminution in the duration of the attack.

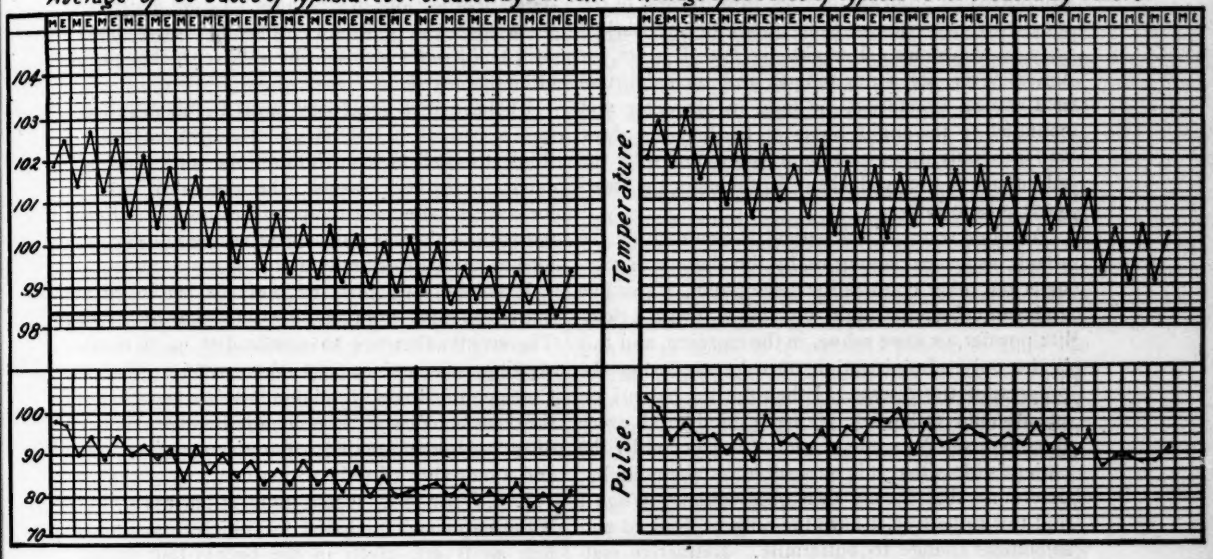
I beg to base my presentation of the subject on the accompanying charts. The first is a composite of thirty cases of typhoid fever, temperature and pulse occurring under my own observation; the second is a composite of thirty cases, temperature and pulse, occurring in the services of other practitioners at the same hospital with myself, and I wish to state that the composite is introduced with the consent of my confrères and solely for the purpose of illustrating my subject. The third chart is taken from the article by Professor William Pepper in his recent American text-book of the *Theory and Practice of Medicine*, and is introduced as a typical chart of typhoid fever. The cases selected to make up the composite are assumed to be average cases scattered over three seasons and are not taken with any view to influencing unduly the result. The composite in each case is obtained by adding the thirty morning temperatures on the day of admission and dividing by thirty and so obtaining an average. The same is done with all the other morning and evening temperatures and pulse-rates. For the accuracy and care with which this has been done I am indebted to my friend Dr. C. D. Nelson, a recent graduate of the Harvard Medical School. The composites would seem to indicate that the average case, in hospital-service, comes under observation with us, as with Professor Osler, at about the end of the first week. Comparing, then, the composite of the thirty cases of my confrères who did not use the Brand treatment, with the typical chart

[1] Read before the Association of American Physicians, Washington, May 29, 1894.

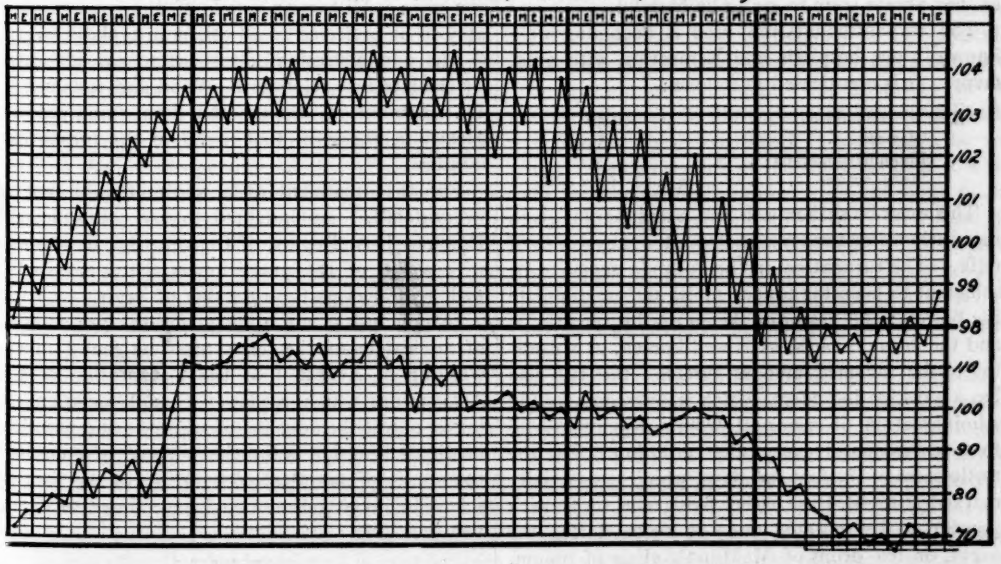
of Pepper, it would seem that the average case of typhoid in Denver is a milder one than prevails in Eastern cities. But our mortality, averaging 1.5 deaths to every 1000 of population, indicates that it is not without danger even here. Even allowing

late Dr. James H. Hutchinson, has justly remarked that "medicines are, however, by no means useless in the treatment of typhoid fever. There is no question that the disease is not only generally conducted to a favorable issue, but that its duration is often

COMPARATIVE CHART



TYPICAL CHART OF TYPHOID FEVER (PEPPER)



that the prevalent type is milder with us than elsewhere, which I am in doubt about, it nevertheless seems, from the composite of my own thirty cases, as though treatment had a decided influence in affecting the type. So careful an observer as the

materially shortened by their judicious use." My own composite would seem to show the justice of this remark. I myself was greatly surprised to see the result of the composite. The steady decline of the temperature from the very outset and a coinci-

dent decline of the pulse-rate are marked. It does not show the ordinary stage of *fastigium* with its two weeks' duration and then the decline by *lysis*. It goes without saying that with this decline in temperature and pulse there is a steady improvement in all the symptoms. The patient, of course, loses flesh and strength. He is not likely to suffer from delirium, from tympanites, from dryness of the tongue, from sordes, or from tremor or any marked nervous symptoms. I might say that I never, or almost never, see a dry tongue, and such delirium as I witness is likely to be mild in character and confined to the night hours of the first few days, and generally disappears in a short time with the subsidence of the headache. In the main the treatment is as follows:

Absolute rest. If the patient is seen early in the attack, as gauged somewhat by the absence of rose-spots and other constitutional symptoms, I give five grains of calomel at night and follow it with a Seidlitz powder, or some saline, in the morning, and this is the only administration of calomel during the progress of the disease. I then put the patient on an absolute milk-diet, giving a tumblerful every three hours. As a rule, I am not influenced by the patient's ideas that it is impossible to take milk, but determine that by my own observation. When I find this to be true, I have the milk peptonized and sometimes change to buttermilk. Extractive matters in the way of broths and extracts are not used, as they always seem to me to be harmful.

The turpentine-mixture used is becoming well-known in our hospitals and is meeting with great favor. It is constituted as follows:

℞.—Ol. terebinthinæ	. . . . .	f℥ij.
Ol. ricini	. . . . .	f℥ij.
Bismuthi subnitratis	. . . .	℥iv.
Mucil. acaciæ	. . . q s. ad	f℥iv.—M.

This must be shaken well and a teaspoonful given in a little water twenty minutes after the goblet of milk. The turpentine in this formula seems to be sufficient to keep the tongue moist and to stimulate the bowels; the castor-oil keeps up the secretions, and the bismuth acts, as is well-known, as a dressing to the inflamed mucous surface; the last also has a constipating effect, which is overcome by the administration of castor-oil, from a dessertspoonful to a tablespoonful every other morning. For the restlessness of the first few days, which is especially marked at night, I use either five grains of Dover's powder, to be repeated once or twice during the night, or ten drops of McMunn's elixir of opium, to be repeated once or twice if needed. Water, iced if preferred, is given freely, and lemonade, not too sweet, is allowed, but not to conflict with the taking of the milk. Antipyretics, like antipyrin, antifebrin, acetanilid, phenacetin, and even quinin (for its antipyretic effects), are absolutely forbidden. Stimulation I reserve for necessity, as indicated by marked nervousness, a rapid and feeble pulse, and for the stage of convalescence. In both instances whiskey is preferred, care being taken to secure a good quality. Of heart-tonics strophanthus is given rather a preference, and I at times resort to hypodermatic injections of strychnin. I have never used the cold baths of Brand; at the utmost, sponging with tepid water seems to be sufficient, though not often employed.

I believe that great care must be taken in feeding a patient in the stage of convalescence. I think it is well to keep him on the milk-diet indicated, increasing, however, the amount if needed, for a week, after the temperature has come to the normal. I then begin to feed very gradually, preferring animal to starchy diet.

It will be seen that the methods employed are quite different from those advocated by others. The strict adherence to a milk-diet is, of course, nothing new, and as far back as 1866 we find Dr. Cotting, of Roxbury, Massachusetts, reporting in the London *Lancet* three hundred and seven cases of typhoid fever treated without drugs of any kind and a mortality of only 10 per cent.

Prof. Osler in his recent publication says: "Milk is the staple article of diet, of which from three to four pints are given in the twenty-four hours." This is about the amount I have used. Dr. Hutchinson says: "Milk as an article of diet is unquestionably to be preferred to all others in typhoid fever."

The administration of turpentine was advocated by the late Prof. George B. Wood and indorsed by Dr. Hutchinson, though the dose employed was greatly in excess of that found in the turpentine-mixture.

For the treatment as a whole, though not in every detail, I am indebted to my colleague, Dr. Edmund J. A. Rogers, of Denver, who, in common with a few of the practitioners there, has used it with great success for a number of years.

In conclusion, my apology for the presentation of an article of this class must be found in the wording of Article I of the Constitution of this Association, which reads: "This Association has for its object the advance of scientific and practical medicine," and it is for the purpose of recording the results of practical observation rather than of scientific investigation that I have dared to detain you thus long.

Dr. Lewellys F. Barker, who has been Fellow in Anatomy at Johns Hopkins University, has been advanced to the position of Associate in Anatomy. He remains also Assistant Resident Pathologist to the Johns Hopkins Hospital.

THE MANAGEMENT OF FACE-PRESENTATION.[1]

BY EDWARD P. DAVIS, A.M., M.D.,
OF PHILADELPHIA;
PROFESSOR OF OBSTETRICS AND DISEASES OF INFANCY IN THE PHILADELPHIA POLYCLINIC; CLINICAL PROFESSOR OF OBSTETRICS IN JEFFERSON MEDICAL COLLEGE; CLINICAL PROFESSOR OF DISEASES OF CHILDREN IN THE WOMAN'S MEDICAL COLLEGE, ETC.

THE frequency of face-presentation in gestation is variously estimated at 1 in 130 confinements, by German observers; 1 in 276, by French; and 1 in 292, by British obstetricians. A hint, I think, may be taken from these statistics as to the methods of obstetric diagnosis employed in the leading maternities of the world. It is quite possible that the greater frequency with which this abnormality is said to occur among German patients is not to be construed into a belief that German women have more face-presentations in labor than do British women, but that owing to the practice of palpation and auscultation before labor, German obstetricians more often diagnosticate the abnormality than do English and French physicians. The importance of early diagnosis of this complication is indicated by the success of spontaneous labor in these cases, and the mortality that attends labor requiring instrumental aid.

In a recent interesting paper by von Weiss,[2] forty-six cases are narrated of spontaneous delivery attended by no mortality. Zeller, in forty cases terminating spontaneously, lost no mothers and but two children. While these statements are not exhaustive as to the advantage of spontaneous delivery, the fact remains that such a termination of labor in face-presentation is most desirable. Methods best adapted to secure this result must be based upon early diagnosis, and hence the importance of perfecting the knowledge and practice of palpation.

The treatment best adapted to secure spontaneous labor in face-presentations consists in retaining the membranes unbroken until the time of spontaneous rupture; of sustaining the patient's strength and conserving her energies by suitable feeding, stimulation, and anodynes; and in placing the patient in such a posture as to favor the rotation of the chin anteriorly. When the face looks toward the left side of the mother's pelvis, she should lie upon the left side, her thighs flexed, and the uterus brought as nearly as possible with its long axis corresponding to the axis of her body during labor-pains. Should the fetal face look toward the right side of her pelvis she should lie upon her right side. The relation of cause and effect that has been found to exist between obliquity of the uterus in the mother's abdomen, and face-presentation, renders the maintenance of the uterus in a favorable posture during this complication a matter of considerable moment. It is quite possible in such cases for a face-presentation to be spontaneously changed into an occipital one, and for the occiput even to rotate spontaneously to the front, if these precautions are observed. The administration of such substances as tend to maintain efficient uterine contractions is certainly indicated in these cases before the membranes rupture. As soon as dilatation is complete, the obstetrician is to choose between radical interference by version, or an effort by the methods of Baudelocque and Schatz to correct the face-presentation by converting it into an occipital one. His choice in this decision should depend upon the size and shape of the mother's pelvis, the comparative size of the fetal head, and the condition of the pelvic floor. When no disproportion exists between the head and the pelvis, and when the pelvic floor, although elastic and resistant, offers no undue obstacle to the mechanism of labor, it is certainly proper to allow the head, under good labor-pains and with good extension, to come down upon the pelvic floor in face-presentation, and thus an opportunity be given for the mechanism of labor to proceed spontaneously. When, however, there is a disproportion between the head and the pelvis, as evidenced by pelvic proportions less than the average, ascertained by pelvimetry, or a large fetal head, as ascertained by palpation and a thorough vaginal examination under an anesthetic, the case becomes quite different.

The results of forcible delivery in face-presentation under these conditions are anything but satisfactory and encouraging. The literature of the subject abounds in illustrations of difficult deliveries by forceps, craniotomy, and, in some cases, by version, in which the life of the child has been lost and the mother seriously injured. In the light of our present knowledge the duty of the obstetrician lies in completely changing the presentation, if the pelvis be roomy, by podalic version under anesthesia by chloroform, or in so enlarging the pelvis that the mechanism of labor in face-presentation can continue.

Podalic version for face-presentation is familiar as an expedient, and is, in well-selected cases, a prompt and efficient method of treatment. Symphysiotomy is a procedure of too recent date to occupy an established place in these cases in the resources of obstetric art.[1] My reasons for considering this a rational procedure are are as follows: Personal observation in four symphysiotomies done for disproportion between the head and the pelvis has led me to remark the decided increase of the oblique diameters

[1] Read before the American Gynecological Society, Washington, May, 1894.

[2] Archiv f. Gynäkologie, 1893, No. 25.

[1] Under date of June 13th, Dr. R. P. Harris writes me, that he knows of no case of face-presentation in which symphysiotomy was performed to rectify the position.

of the pelvis that follows the separation of the pubic joint. In three of the four cases, the fetal head presented in an occipito-posterior position; in two cases the right occipito-posterior, in one the left. In one of these cases the head presented by the right parietal bone before the pelvis was opened. Remembering the observation by many obstetricians that face-presentations are often developed out of occipito-posterior positions, it seems to me rational that any surgical procedure which should enlarge the oblique diameters of the pelvis, and which naturally favors the descent of the occiput in the arc of the pelvic curve lying between the sacro-iliac joint and the spine of the pubes, would afford rectification of this abnormal presentation. The three cases of occipito-posterior position following symphysiotomy were readily delivered without injury to mother or child.

Obstetricians are familiar with the facility with which faulty presentations of the head and shoulders may be remedied when two conditions are present: first, complete anesthesia of the mother; and second, a pelvis of good size in its oblique diameters. Reasoning from my experience in the cases cited, these conditions are present after symphysiotomy; and were the spontaneous progress of labor in this presentation to fail, I should much prefer, if the pelvis were of good size and the head not impacted, to perform podalic version; or, if the head were impacted, and disproportion existed between the head and the pelvis sufficient to occasion difficulty in labor, the face-presentation persisting, I should hope for a favorable result from symphysiotomy, converting the face-presentation into a vertex-presentation, and delivering the occiput in the manner most easily available after the pubic joint had been opened.

MEDICAL PROGRESS.

Hydatid Cyst in the Neck of a Child.—SMITH (*New Zealand Medical Journal*, vol. vii, No. 2, p. 88) has reported the case of a girl, four and one-half years old, who presented a tense, elastic swelling, as large as an orange, obviously containing fluid, situated beneath the angle of the jaw on the right side, and completely filling the middle triangle of the neck. The skin over the tumor was smooth and non-adherent, and presented no indication of inflammation. The formation was believed to be a "cold" glandular abscess, but an exploratory incision proved the diagnosis to be incorrect. Accordingly an incision was made and the tumor was found to be encapsulated and to contain fluid. Spread out over its deep attachment were the thinned and barely recognizable fibers of the posterior division of the sterno-mastoid muscle. Posteriorly, the tumor was intimately adherent to the sheath of the common carotid artery, displacing to one side the omo-hyoid muscle. Above, it penetrated behind the angle of the jaw and beneath a portion of the parotid gland. In front, it was adherent to the fibers of the sterno-hyoid and sterno-thyroid muscles. Several nerves, including the descendens noni and the hypoglossal, were dissected from the posterior surface of the cyst and all veins were divided between two pairs of pressure-forceps. The only vessel requiring ligation was a branch of the superior thyroid artery. The intimate relations of the tumor rendered its dissection difficult and laborious. On incising the cyst-wall, which was very thick and quite characteristic, it was found to contain clear fluid and a number of smaller cysts, microscopic examination of the contents of which disclosed the presence of hooklets. The patient made an uninterrupted recovery and was dismissed in a fortnight.

Paralysis Agitans without Tremor.—At a recent meeting of the New York Neurological Society, Dr. Frederick Peterson presented a wheelwright, sixty years old, who displayed characteristic mask-like rigidity of the face, with the attitude of paralysis agitans, the stooping shoulders, bent back, crooked elbows and knees. The hand and fingers were held in the characteristic position, but there was no tremor anywhere in extremities or head. The voice had the monotony and something of the festination of the speech of paralysis agitans. There was at times a sort of propulsion in walking.

THERAPEUTIC NOTES.

The Mechanical Treatment of Constipation.—As the result of an elaborate study, based upon a large experience, MARINEL (*Journal de Médecine de Chirurgie et de Pharmacologie*, 1894, tome iii, fasc. 1, 2, p. 1) concludes that the mechanical treatment of constipation constitutes a therapeutic measure acting with great energy upon the circulation, the respiration, and the general nutrition. It brings about a modification of the abdominal circulation, and causes the disappearance of certain conditions of passive congestion, notably those encountered in cases of abdominal plethora. Under its action both the volume and the force of the muscles is increased. It is the best curative agent in the treatment of constipation dependent upon paresis or paralysis of the muscular system of central nervous origin, and also when the constipation is dependent upon hypesthesia or anesthesia of the intestinal mucous membrane of local origin. It is contra-indicated when constipation is due to acute inflammatory lesions or to new-growths.

For Whooping-Cough.—

℞.—Sulphonal . . . . . gr. j.
Creasoti . . . . . ♏ij.
Syrup. tolu. } . . . . . ãã f ℥ij.
Aquæ }

S.—Two teaspoonfuls every two hours.

Practitioner.

For Lead-poisoning PEROU (*Lancet*, No. 3690, p. 1277) recommends sodium monosulphite in doses of six grains daily. By this means speedy relief from colic is obtained, and the elimination of the lead is also hastened. The agent is besides an efficient prophylactic and its use is unattended with danger.

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SATURDAY, JULY 14, 1894.

HYPNOTISM AND CRIME.

IT has long been apparent that hypnotism was to have sooner or later its medico-legal aspects. When EYRAUD, the strangler of Paris, committed his foul crime he used as a sexual decoy a young woman named BOMPARD. The victim was taken by EYRAUD *en flagrante delicto* with BOMPARD, and so between the two he was neatly strangled. At the trial the astonishing claim was made that BOMPARD had been hypnotized by EYRAUD, to be his accomplice, and this claim was pressed in court to such purpose that the prisoner—probably because she was a woman—was merely imprisoned instead of being guillotined. Specialists in hypnotism (for there are such specialists in France) urged that the process in BOMPARD's case had been by *suggestion;* and a critical outside world exclaimed that almost all evil had been by suggestion ever since the first séance between EVE and the serpent.

It is interesting to note now the appearance of an ambitious treatise by CROCQ[1] on *Hypnotism and Crime,* in which this author opposes the theory of an hypnotic influence in the case of BOMPARD, and proceeds to work out in his own way both a theory and its application for this somewhat abstruse subject. After an admirable historic survey, CROCQ defines hypnotism as a "particular state" developed under the influence of the concentration of all the intellectual faculties upon one point. This hypnotic state is inseparable from an initial hypnotic sleep, which may manifest itself as well in healthy as in unhealthy or hysterical subjects. It presents various stages, all allied to a somnambuloid state. In this particular mental state suggestibility is a marked feature, *i. e.*, the subject may be led to commit automatically a variety of acts, including criminal deeds, without always retaining a distinct recollection of them after returning to the norm.

It will be seen at once that this is a more critical and exact statement than that of the school of Nancy, which in BOMPARD's case defined hypnotism to be practically identical with suggestion. It is one thing to say that suggestion is hypnotism, but a vastly different thing to say that hypnotism is a special state in which suggestion may act with increased force. The first proposition is untenable, because practically it reduces all influence of mind upon mind, hence most of education and much of morals, to the level of a psychosis; the second is scientific, because it affirms merely that in a special psychosis, *i. e.*, the hypnotic state, suggestion has a pathologic significance. So far we think the statement of CROCQ is exact and the theory valid. The exceptions that we take to this clever and intellectual French (or, to be accurate, Belgian) treatise have reference to some refinements of definition and to some of the applications of the theory in special cases.

CROCQ is inclined to draw too fine a distinction between "hypnosis" and some of the well-recognized psychoses—hysteria, for example. We demur to this, especially when the author claims that hypnotism can exert an influence upon perfectly healthy minds, unless he is willing to include the various grades of hysteria within the limits of perfect health, which is doubtful. We think that the genuine hysterical state is closely analogous to, if not identical with, a condition of hypnosis. We see in hysteria that kind of hypnosis known as double consciousness, with contraction of the field of consciousness and extreme suggestibility. Wherein does this differ from hypnotism? In nothing, except that one is a condition, the other a method; one is inherent, the other induced; one is natural, the other artificial—vain distinctions, from the critical standpoint.

[1] "L'Hypnotisme et le Crime." Par le Dr. Crocq, Fils. Bruxelles: Henri Lamertin, Editeur. 1894.

The practical part of CROCQ's book lies in its application. Crimes are considered *seriatim*. Rape, theft, the substitution of infants, false testimony, murder, and suicide are the chief among these. The quality of this part of the book cannot be judged by an isolated quotation, but one case may be cited as illustrating the author's acumen and naïveté. A young woman, who was hysterical and somnambulistic, left PITRE's service at Bordeaux, "absolutely and surely a virgin." She was accompanied by another hysteric. They soon encountered two gentlemen, who took them to breakfast. Hysteric number one was embraced by one of the gentlemen, in spite of the fact that she became violently angry. She defended herself; she even *threatened* to cry out. But it was useless. Her suitor hypnotized her; she lost consciousness, and "*Neuf mois après ce jour la jeune fille accouchait d'un enfant à terme.*" This is a clear and logical conclusion. The simplicity of the tale is exceeded only by the simplicity of the mind that finds in it the evidence of a psychosis.

All of CROCQ's examples are not of the same sort as this, and some have in them more evidence of the mental state that is now such a favorite object of study in Paris. When, however, we think of one of our average American courts or juries devoting itself to a conscientious study of this book we experience a hope that the future translator will edit carefully the illustrative cases. The ordinary American juryman would not put faith in a virgin "absolutely pure," who picked up a strange man on the street and held with him a salacious "hypnotic" séance in a neighboring saloon. The verdict, we fear, would not sound well to ears polite.

It may be for the modern Athenians to take the lead in this new province of medical jurisprudence; and until we see how they manage these things in France we shall rest content with the admirable light from M. CROCQ's book.

EDITORIAL COMMENTS.

Fever without Pyrexia.—We have on previous occasions called attention to the apparent paradox of the occurrence of febrile conditions without elevation of temperature. In the course of a recent clinical lecture, TEISSIER (*Semaine Médicale*, No. 25; *Wiener med. Presse*, 1894, No. 23, p. 893) presented a case of pneumonia attended with a subnormal temperature, and made some interesting comments. He pointed out that the temperature may pursue a similar course in undoubted cases of scarlatina, as well as in cases of typhoid fever and influenza. He also made reference to two cases of typical malarial fever attended with subnormal temperature. These were not cases of pernicious malaria of algid type, but were intermittents of moderate intensity, and in which quinin brought the temperature to the normal. Several explanations of this anomalous occurrence have been offered. One of these makes it dependent upon the varying reaction of the thermotaxic centers to varying degrees of virulence of the infecting organisms, a slight degree inducing hyperthermia and an intense degree hypothermia. The inadequacy of this explanation is evident from the fact that it is not always the gravest cases that are attended with hypothermia, and *vice versa*. Rather more plausible is the explanation that pathogenic microorganisms give rise to the production of substances that cause elevation and of others that cause depression of temperature. Finally, it is possible that the hypothermia is due to the accumulation in the body, from failure of elimination, of excrementitious products by reason of hereditary tendencies or of renal inadequacy. The action of quinin in causing elevation of abnormally low temperature is to be ascribed to its vaso-constrictor action, thus preventing undue radiation of heat. Among other measures to be adopted is the stimulation of the cutaneous and renal activity. All in all, the subject is a most interesting and suggestive one. It would seem as though we would have to modify, or at least qualify, the current conception and definition of the febrile process. The problem here propounded is but one of many for the solution of which we must look to chemic physiology.

The Judicial Council.—"Our esteemed contemporary, the Philadelphia MEDICAL NEWS, states that the question of the 'conduct of *The Journal*' was referred to the Judicial Council and has not yet been decided by them.

"At the last meeting of the Association, the question of the action of the management of *The Journal* in publishing certain advertisements was considered by the Judicial Council, and after a full hearing in the matter, the Council decided to leave it as heretofore in the hands of the Trustees."—*The Journal of the American Medical Association*, July 7, 1894.

Our cotemporary is characteristically inexact. Its insertion of the words "by them" after the word "decided" alters the meaning of our statement. We said that "the question of the conduct of *The Journal* is not yet decided"; and that "for the present discussion may well be suspended." We repeat the statement.

We also said that we were not yet informed as to the action taken by the Judicial Council on the points submitted to the Council by the MEDICAL SOCIETY OF THE STATE OF PENNSYLVANIA, and by the resolution of DR. EDWARD JACKSON referred at San Francisco. Our cotemporary but partially enlightens us. Until the full text of the decision is published, we cannot assume that our cotemporary correctly interprets it, seeing that our cotemporary sometimes makes errors of interpretation in other matters; nor can we comment upon the decision, whether or not our cotemporary is correct in its interpretation thereof, until the reasons therefor are before us. We await the full text with much interest and trust it will be promptly made public.

Protective Inoculations against Cholera.—The success of the method of inoculation devised and practised by M.

HAFFKINE as a means of protection against cholera is being put to a practical test in India. Within a year or thereabouts some 25,000 persons have been already thus inoculated, but hitherto the crucial experiment of exposure to an epidemic of the disease had not been made. It is now authoritatively reported that in a community of two hundred inhabitants near Calcutta, in which cholera broke out, and of whom one hundred and sixteen submitted to the inoculations, ten persons, none of whom had been inoculated, were attacked and seven died. Of a family consisting of eighteen persons in which a fatal case of cholera occurred, eleven submitted to inoculation. When cholera again broke out in the same house at a later date, four persons were attacked, none of whom had been inoculated, and three died. In another instance four of six members of a family were inoculated several months ago; recently when cholera appeared one of the two uninoculated was attacked, while all of the inoculated escaped. In a family of eleven, five were vaccinated; one of the six not inoculated was attacked. Six of a family of nine were inoculated; one of the three not inoculated was attacked. In all instances those inoculated escaped. The number of cases thus far treated it will be admitted is too small to formulate final conclusions upon, but the results are, to say the least, encouraging and the hope will be universal that they may be abundantly confirmed by more extended observations.

Milk-inspection.—The practical advantages of an intelligent and systematic inspection of milk for domestic use are admirably illustrated by the report of the Inspector of Milk of the city of Boston, who announces a distinct improvement in the quality of the milk served to families as a result of the vigilant enforcement of the inspection-laws. The fines imposed for failure to conform to the standard established were more than sufficient to pay the expense of the inspections. Two things, however, are absolutely essential for successful results in this connection: (1) the enactment of practical and intelligent legislation, and (2) its faithful and rigorous execution.

REVIEWS.

THE HEALING OF RODENT CANCER BY ELECTRICITY. By J. INGLIS PARSONS, M.D., M.R.C.S., M.R.C.P. (Lond.), etc. Small 8vo, pp. 82. London: John Bale & Sons, 1893.

THE title of this little book would be more correct if the word "destruction" were substituted for "healing." For the author distinctly says (p. 10): "My aim has been to adjust the strength of the electric current so as to kill the malignant cells, yet not to injure the healthy cells beyond recovery." When the morbid structures have been completely extirpated, whether by the knife, by caustics, or by electricity, healing may be expected to take place as in an ordinary simple ulcer.

After a discussion of the pathology and clinical characters of the disease (based, we judge, upon a communication by Mr. Jackson Clarke, on "Parasitic Microorganisms in Cancer," in vol xliv of the *Transactions of the Pathological Society of London*), a very good "outline of elementary principles for the benefit of those who have not studied electricity" is given. The advantages of this agent are next expounded, and then the author's method of using it is explained. Finally, the details of four cases are presented. So far as appears, these comprise the whole of the author's experience in this direction.

Very possibly a surgeon unfamiliar with the employment of electricity, if called upon to treat a patient with rodent carcinoma, might find some assistance in these pages.

THE TECHNIQUE OF POST-MORTEM EXAMINATION. By LUDVIG HEKTOEN, M.D., Pathologist to the Cook County Hospital, Chicago; Professor of Pathologic Anatomy in the College of Physicians and Surgeons of Chicago. With 41 illustrations. 8vo, pp. viii, 172. Chicago: W. T. Keener & Co., 1894.

IN this little book Dr. Hektoen reflects the good pathologic work that he is doing in Chicago. Perhaps the greatest defect of the American medical curriculum has been in the department of practical pathology, and it is gratifying to notice a growing tendency to improvement in this direction. The fault lies in some degree in custom and education, for post-mortem examinations are far less common in the United States than they should be. The work of the Coroner's office in most cities is not calculated to invite the admiration of the earnest pathologist, either in its execution or in its results, and we take this occasion to express the hope that the future has better things in store for scientific medicine. Dr. Hektoen has provided us with an excellent guide in post-mortem examinations, which we can heartily commend to both students and medical men. Every student should be familiar with this technique, not only from reading, but also from observation and practical experience.

THE MICROSCOPE AND MICROSCOPICAL METHODS. By SIMON HENRY GAGE, Associate Professor of Anatomy, Histology, and Embryology in Cornell University. Fifth edition; rewritten, greatly enlarged, and illustrated by 103 figures in the text. Part I. The Microscope and Histology. Pp. 165. Ithaca, N. Y.: Comstock Publishing Co., 1894.

THIS new edition of a most useful book has been enlarged by the addition of a new chapter on photo-micrography and allied matters. The number of illustrations has also been considerably increased. Their distribution in the text is an advantage over grouping a number together. We need not repeat the words of commendation it gave us pleasure to express in reviewing the third edition (THE NEWS, April 30, 1892, p. 502). The book remains a most practical and intelligent guide for the beginner, who so often finds difficulty in becoming well grounded. The work bears the impress of the laboratory, and it is largely on this account that it commends itself. We await with much interest the appearance of the second part of the work, which, it is announced, is in preparation and may be expected in the course of two years.

Prof. Madelung has been appointed Professor of Surgery in the University of Strassburg, in succession to the late Prof. Lücke.

SOCIETY PROCEEDINGS.

AMERICAN PEDIATRIC SOCIETY.

Sixth Annual Meeting, held at Washington, D. C., May 29, 30, 31, and June 1, 1894.

FIRST DAY—MAY 29TH.

THE meeting was called to order by the First Vice-President, DR. FORCHHEIMER, of Cincinnati.

DR. B. K. RACHFORD, of Newport, Kentucky, read a paper entitled "The Influence of Venous Congestion on the Spinal Reflex Centers." He maintained that three important changes in the blood bear an etiologic relation to the neuroses of childhood, viz.: (1) the presence of toxins; (2) simple anemia, acute or chronic; (3) venous congestion, acute or chronic. These do not exist as separate etiologic conditions, but as factors of the complex blood-condition that has much to do in producing the excitability associated with the chronic anemias of childhood. He called attention to the fact that toxic products may produce nervous symptoms, and may therefore be etiologically important in the neuroses of childhood. There are other toxins in the body besides those of bacterial origin. Paraxanthin is a poisonous leukomaïn of the uric-acid group, and is an etiologic factor in a group of nervous symptoms that has heretofore been ascribed to uric acid. Dr. Rachford starved and bled a number of rabbits into a condition of profound malnutrition, and was led to suspect that the blood-changes in scrofula, malaria, rheumatism, and other blood-diseases might not be simply the result of anemia, but that the blood might also contain effete and poisonous products that might also help to keep the nervous system in a state of chronic irritability. This view had been confirmed by ligation of the abdominal vena cava in the rabbit. This experiment had shown that a most exaggerated reflex irritability of the spinal cord might be thus produced, but that the reflex irritability did not begin until the end of the second week after the operation, and that from this time on for a period of six or eight weeks, it continued to increase. Too much stress is not to be laid on this as indicating excitability of the spinal cord, as it might have been due to the changed condition of the muscles, and nerves, and cells, and not to the state of the cord. The same results were obtained in the rabbit by ligature of the abdominal aorta. The reflex phenomena produced by starvation were very insignificant when compared with those produced by venous congestion or arterial anemia. It seems fair to conclude, therefore, that venous congestion and arterial anemia do not act solely by malnutrition, but that there are additional factors, and that these are to be found in the effete and poisonous products of metabolism kept in contact with the nervous system instead of being eliminated. It seems probable that a similar explanation might be applied to the development of chorea and other nervous affections in children in the sequence of endocarditis and pericarditis.

DR. T. M. ROTCH, of Boston, said that from a clinical standpoint he was thoroughly in harmony with the views expressed. Almost every clinical observer must have noticed the relation between a feeble circulation and the existence of nervous disorders.

DR. AUGUSTUS CAILLÉ, of New York City, read a paper on "Tonsillotomy followed by Diphtheria and Croup." He reported the case of a boy, four years old, who had been brought to him for the removal of hypertrophied tonsils. Adenoid vegetations were found on digital examination, and the gums were somewhat swollen from caries of the molar teeth. At the request of the parents, who lived at some distance from New York, Dr. Caillé decided to operate at once, without carrying out his usual prophylactic treatment. Both tonsils were removed with a disinfected tonsillotome. The parents were instructed to use Labarraque's solution. The boy was also given internally potassium chlorate every hour, as he was unable or unwilling to gargle his throat. The next day he was violently sick. The stumps of both tonsils were covered with pseudo-membrane, and there was vomiting and cerebral symptoms. A culture was made from the pseudo-membrane and from the carious teeth, and this showed Loeffler bacilli and streptococci—a mixed infection. The boy was given a spray of mercuric-chlorid solution, to be applied to the mouth and pharynx every half-hour. He was also given internally minute doses of mercuric chlorid. The voice became somewhat husky. On the next morning a small streak of diphtheric deposit had appeared on the posterior pharyngeal wall, and a croupy cough was well marked, while the epiglottis was highly congested. Twelve hours later, the posterior surface of the epiglottis was covered by a thin deposit, and the laryngeal stenosis had increased. Intubation was performed on the following morning. The internal administration of mercuric chlorid was discontinued, but a spray of a saturated solution of boric acid was continued. There was a purulent discharge from the tube, and this was removed. The pseudo-membrane deposited on the tonsillar pharynx remained visible a few days longer. The boy ultimately recovered, and three weeks afterward the adenoid vegetations were removed from the pharynx.

DR. A. JACOBI, of New York City, said that for twenty years he has been teaching the importance of not operating during the prevalence of diphtheria in a community, and especially in cases in which the tonsils are to be removed. Hundreds of times had he seen the wound of an exsected tonsil covered with a thick pseudo-membrane, and in many instances this had been followed by general diphtheria.

DR. H. KOPLIK, of New York City, expressed surprise at the large number of tonsils that harbored the Loeffler bacilli in the very depths of the lacunæ. Children with large tonsils are very prone to attacks of lacunar amygdalitis. Virulent diphtheria-bacilli were found in the depths of the lacunæ—not on the surface. By making a culture from the tonsils previous to doing any operation upon them, one can, after the lapse of forty-eight hours, tell positively whether diphtheria-bacilli are present or absent. Diphtheria does not usually develop when no operation is performed on the tonsils, because as long as the diphtheria-bacilli remain in the lacunæ they are bathed in the lymph which exudes into the lacunæ, so that they are under the influence of leukocytes, and their virulence is diminished. Non-virulent bacilli replace the virulent form after the bacilli have remained in the tonsil several weeks.

DR. DILLON BROWN, of New York City, agreed as to

the importance of using antiseptic lotions both before and after naso-pharyngeal operations; in certain cases, however, it is difficult to carry out this precaution. Under such circumstances the use of antiseptic vapors, such as the fumes of sublimed calomel, is to be advised.

DR. F. HUBER, of New York City, read a paper entitled "Congenital Constipation." He said that beside constipation from other causes, there is a congenital variety dating from the first hours of life, and dependent upon the normal anatomy of the colon. The alvine evacuations are dry and insufficient, the watery elements having been absorbed in the reduplication of the colon. Dr. Huber reported the case of a boy, five weeks old, who from birth had suffered from constipation and cramps. The child was healthy in appearance, but evidently suffering great pain. He was straining, kicking, and crying. He had vomited for two days; the abdomen was swollen and tympanitic, and there was an indistinct, elongated swelling to be felt across the abdomen, two inches above the level of the umbilicus. His temperature was 102° F. By a process of elimination and exclusion, a diagnosis was made of acute intestinal obstruction from birth, with impaction of feces in the reduplication of the sigmoid flexure. In many cases the affection is trivial, but in others it is very serious, and might even jeopardize life. The ascending and transverse colon being very short in the fetus and the newborn, and the descending colon very long in proportion, the latter is crowded down into a narrow pelvis, and thrown into a number of curvatures instead of into one sigmoid flexure. The intestinal contents are therefore retarded, the fluid absorbed, and the feces, becoming hard and dry, give rise to obstruction. The treatment consists in distending the lower bowel with fluid, whereby the reduplications are gradually unfolded from below, and the patency of the gut is restored. High rectal injections should be given, and it might be necessary to continue this treatment for years before the proper relation has been established between the colon and the sigmoid flexure. Crying or any increase in the abdominal pressure should be carefully avoided.

DR. W. L. STOWELL, of New York City, said that by a persistent use of ox-gall and sweet oil, and sometimes turpentine, when hard scybalous masses are present, either in infants or adults, he had always been able to move the bowels in these cases.

DR. FRUITNIGHT said that as many of these cases of constipation are due to an unusual dryness of the feces, the free administration of water should not be neglected, in order to favor the solubility of the alvine contents.

DR. J. C. WILSON, of Philadelphia, read a paper entitled "A Case of Malignant Measles," which is to appear in a future number of THE NEWS.

DR. FORCHHEIMER requested that in the discussion the frequency of second attacks of measles be considered.

DR. ROTCH placed himself on record as believing that measles could occur several times in the same individual.

DR. FRUITNIGHT said that he had seen measles three times in the same person, occurring between the second and fourteenth years.

DR. STOWELL said that in a large dispensary practice he had seen a great many cases of measles duplicated.

DR. FORCHHEIMER stated that he had found the occurrence of second attacks of measles in the same person very common, and it was not uncommon to have a third attack, not to say anything about German measles occurring in the intervals between these attacks.

DR. SEIBERT said that in a very extensive dispensary practice he had never had the opportunity of making the diagnosis twice in the same child. While believing, therefore, that it was possible for the disease to occur twice in the same individual, he thought it was not nearly of so frequent occurrence as some would have us believe.

DR. WILSON, in closing the discussion, said that he had had associated with him in consultaton in the case he had reported, Drs. Pepper and Da Costa, and they had all agreed as to the diagnosis.

SECOND DAY—MAY 30TH.

As part of the discussion on "The Etiology, Prevention, and Treatment of Rickets," DR. IRVING M. SNOW, of Buffalo, read a paper entitled "The Influence of Race, Especially the Italian Race." He said that in the community in which he practised, rickets was six times more prevalent among Italians than among the other classes, and that the disease prevailed but to a moderate extent among the children of the English, Germans, Irish, and Poles. He thought that the reason for this was to be found in the change of environment, which was so great for the Italian, who in his native land had lived in a mild, clear climate, with plenty of sunshine, and out of doors for most of the year—a climate very different from that of Buffalo. The change had been too sudden and too severe. The mean winter temperature of Buffalo is 27.15°, and that of Naples 48°. In spring, the mean temperature of Buffalo is 43°, that of Naples 58°. It is certain that rickets is seldom developed among the natives of Southern Italy, Egypt, and India, and this fact is probably accounted for by the out-of-door life of the people and the bright warm sunshine of the climate.

DR. GEORGE N. ACKER, of Washington, D. C., read a paper on "The Influence of the Negro Race." He referred to the many peculiarities of the negro race and the differences between it and the white race. He said that since the war the conditions affecting the negro had been entirely changed. The slaves had food conducive to health and were warmly clad; they lived in healthful houses and were cared for when sick. Now the negroes have to work for themselves; they are shiftless and intemperate; they breathe impure air; wear insufficient clothing, and live in total disregard of all sanitary rules. All this has necessarily exerted a deleterious effect upon their offspring. The race is undergoing serious decay. The diagnosis of rickets can easily be made in the negro if search be made for the usual evidences of the disease. The bone-lesions often appear late and are not always found, so that some severe cases are overlooked. A most important etiologic factor is heredity. The parents are as a rule tuberculous or syphilitic. In Dr. Acker's experience it is common for children with the tuberculous or syphilitic diathesis to develop severe rickets. On account of their unsanitary surroundings and the lack of care given them, the health of their offspring was liable to become considerably impaired. If breast-fed, they failed to receive nourishment at regular hours, as most of the mothers were in service, and other and improper food was given. The negro infant is weaned at an early age; it is not uncommon to find

infants of a few months old fed on condensed milk, oatmeal, beef-tea, bread and butter, and tea. Between the sixth and eighteenth months of life pulmonary disorders are very common, and it is at this time of life that rickets is most frequently observed. Disease of the respiratory organs lowers the vitality, and in this way might be considered an etiologic factor in causing rickets. The pathologic changes found in rickets are about the same in negroes as in other races. Enlargement of the wrist-joint is more prominent; the swelling of the ribs occurs earlier and is more constant; the thoracic grooving is marked in all cases. The head is rectangular, but this is not a constant feature. In some cases the fontanels close early and the skull is firmly ossified. Although Dr. Acker had made careful search for cranio-tabes, his experience has led him to think it quite rare among the negroes. As negroes have less susceptible nervous systems than whites, they are less liable to nervous diseases. When these rickety children are placed in more favorable surroundings the disease usually yields quite readily to treatment.

DR. C. W. TOWNSEND, of Boston, reported "A Case of Congenital Rickets." The child was seen at the Boston Lying-in Hospital, and was born of healthy parents who had no syphilitic history. A previous child had been born to these people and up to the age of three years had shown no signs of rickets. The parents were wretchedly poor; the child was a male, premature, and weighed seven pounds. His head was sixteen inches in circumference; the sutures were widely open; there was a large area of cranio-tabes; Wormian bones were present; there was considerable cyanosis, which might have been partially due to an open ductus arteriosus. No murmur was heard over the heart. There was the characteristic rickety rosary; the abdomen was large; the spleen could not be felt; the extremities showed evidences of rickets, enlargement of the epiphyses being easily noticeable. The bones of the forearm were bent posteriorly; the femur was curved outward and forward. These curvatures could only be explained by the action of the muscles on the soft bone *in utero*. There were also fractures due to the extreme brittleness of the bones, a late phase in the rickety process, and therefore they must have been produced by muscular action within the uterus. The child was artificially fed, and died on the ninth day. There was no autopsy.

DR. J. P. CROZER GRIFFITH, of Philadelphia, remarked that he had not been able to observe any difference in the causation of rickets in the colored and in the white race. It seemed to him reasonable to suppose that at least the colored children of Philadelphia should have become acclimated by this time.

DR. L. EMMETT HOLT, of New York, said that the most severe cases of rickets seen in New York are among the negroes and Italians. The causes of rickets are many and far-reaching. The predisposition of these two races seemed to be unquestioned, and he has been led to about the same conclusions as those reached by Dr. Snow.

DR. J. HENRY FRUITNIGHT, of New York City, said that it would seem that the nations of Continental Europe are more liable to rickets than are the inhabitants of the British Islands. He also mentioned a case of congenital rickets which he had seen, thus making four cases on record in this country.

DR. A. SEIBERT, of New York City, called attention to the fact that in by far the majority of cases of rickets he had found cranio-tabes since he had made it a practice to especially look for this condition. He said that it should be especially looked for if there is any history of eclampsia.

DR. JOSEPH E. WINTERS, of New York City, said that climate has an influence in producing rickets, but that it is not climate alone, but the way in which it influenced assimilation and digestion. Every one of these cases, notwithstanding the change of climate, could be cured with proper food. With reference to tuberculosis and rickets, Dr. Acker had stated that tuberculosis and rickets were associated conditions. Dr. Winters said he had yet to see a tuberculous child become rickety. He thought that if a child suffering from tuberculosis were subjected to the conditions that induce rickets, the child would die before rickets had developed.

DR. H. KOPLIK, of New York City, said with reference to the value of drugs in the treatment of rickets, that he favored the administration of phosphorus in the daily dose of $\frac{1}{120}$ of a grain, and he thought this treatment led to very satisfactory results if persistently kept up.

DR. ACKER, in closing the discussion, expressed his opinion that if placed under proper surroundings the negro would be cured of rickets without resort to any drugs.

DR. A. SEIBERT, of New York City, read a paper entitled "An Aid to the Sterilization of Milk in Artificial Infant-feeding." He called attention to a method of cleaning milk before sterilization, thus enabling everyone to remove the gross filth from the milk, and at the same time diminish the number of bacteria contained therein to a remarkable degree. He had made a number of experiments that showed that seven-eighths of the bacteria were removed from a given bulk of milk by filtering through absorbent cotton. He had also experimented to ascertain whether the amount of cream or the specific gravity of the milk was changed, and he had found that the milk was not altered by the filtration through a one-half-inch layer of compressed absorbent cotton, and also that the quantity of sugar was not diminished. At his request, Dr. Otto Kiliani, of New York City, had made some control bacteriologic experiments. These experiments showed beyond a doubt that filtering the milk reduced its germinating capacity considerably, and that the purifying quality of the cotton was decidedly intensified by moistening it before filtration. By this process Dr. Kiliani had been able to reduce the bacteria to one-twentieth of their original number. A quart of milk could be filtered in from twelve to fifteen minutes. All the apparatus necessary is an ordinary funnel in the bottom of which the absorbent cotton is packed.

DR. KILIANI said that his share of the work had only consisted in performing the control-experiments alluded to. As a proof of the care with which these were conducted, he mentioned the fact that out of nineteen experiments not a single culture had been obtained as a result of infection from the hands or from the air.

DR. SEIBERT said that in presenting this method for purifying milk he did not wish to be understood as opposing sterilization in the ordinary manner. On the contrary, he would now sterilize more than ever, because

he knew that he could give his patients more completely sterilized milk than he had hitherto been able to do.

DR. J. HENRY FRUITNIGHT, of New York City, read a paper entitled "Infantile Scurvy, Especially its Differential Diagnosis." He said that since the writings of Barlow and Northrup, and others, infantile scurvy had been much more frequently diagnosticated than before. As scurvy is a disease resulting from faulty nutrition, it could be safely assumed that more cases are encountered now than when breast-feeding was more general. He described in detail a typical case of infantile scurvy. When this child first came under observation it was in a deplorable condition. There was a fusiform swelling of the left ankle and a swelling of the thigh on the same side, both of which were very painful; the temperature was 100°; the pulse feeble and very rapid; the gums dusky-purple; the tongue and cheeks were ulcerated, and the whole mouth was inflamed and emitted a fetid odor. The slightest touch induced bleeding from the gums. There were petechial spots over the back and limbs. The right eye was closed by ecchymosis; the conjunctiva was involved in this effusion, and the swelling extended far down on the cheek. There was considerable blood in the stools, but the child did not have diarrhea. It was extremely anemic. Although it was evident that the prognosis was not very encouraging, treatment was instituted, and the child recovered completely at the end of six or seven weeks. The chief point in treatment was the change of food, Pasteurized milk being ordered, together with orange-juice and citrate of iron. In recovering, the patient first improved in the leg, then in the mouth and gums, and the hemorrhagic spots were the last to disappear. Dr. Fruitnight believed that the condition of malnutrition present in scorbutus could not but exert a special influence on the bloodvessels. The most important cases of scurvy had not been rickety, although the disease had been formerly diagnosticated as acute rickets. It was an error to suppose that scurvy never occurred in breast-fed children. The chief points on which the diagnosis was made were the swelling and tenderness of the ends of the femur, the swollen and spongy condition of the gums, and the rapid and complete cure that followed a resort to antiscorbutic treatment. The condition might be mistaken for acute rheumatism. In scurvy the joints are white and tense, but not puffy; in rheumatism the joints are red and puffy, and the tenderness is localized in certain spots. In scurvy there is no local elevation of temperature; in rheumatism there is such an increase in the local temperature. The diagnosis is, of course, more difficult when only poorly-marked joint-symptoms are present, and here the therapeutic test should be resorted to to decide the question of diagnosis. It had been said that this would expose the patients to endocarditis by delaying the treatment if the case were one of rheumatism; but if the symptoms do not quickly abate there is sufficient reason for believing the case to be one of rheumatism, and the appropriate treatment for this disease could then be immediately adopted. The pseudo-paralyses found in infantile scurvy require to be differentiated from other infantile paralyses. They could only occur from bone-lesions in scurvy, and these should be looked for. Scurvy might be looked upon as the result of a faulty nutrition leading to the deviation of the normal constitution of the blood, probably as a result of deficient alkalinity. It is also associated with changes in the bloodvessels. The three important symptoms are the bone-lesions, the spongy condition of the gums, and the petechiæ. The symptoms appear first in the lower extremities, then in the gums, and lastly the hemorrhagic extravasations appear. The disease usually runs a favorable course under a proper dietetic treatment.

THIRD DAY—MAY 31ST.

DR. W. S. CHRISTOPHER, of Chicago, read a paper entitled "The Nutritional Element in the Causation of Neuroses." He said that the following positive factors are present in all neuroses: (1) heredity; (2) anemia; (3) some local condition acting reflexly as the immediate exciting cause. Clinically, the nutritive quality of the blood can only be determined by an estimation of the quantity of hemoglobin and the number of blood-corpuscles present in a given quantity of blood. Such an examination, however, does not furnish very satisfactory evidence. A neurosis was described as a group of phenomena resulting from such abnormal action of tissue or tissues as might be dependent upon one or more of the following conditions: (1) abnormal chemic constitution of the tissues; (2) abnormal chemic composition of the nutritive elements supplying the tissues; and (3) derangement of the nerves. Each individual requires not only a certain quantity of food but also a certain variety, in order to meet the various chemic requirements of the human organism. The proteids, carbohydrates, and fats do not supply the needs entirely. Aside from salts and water, there are other things that probably act as food. The nutritional antiscorbutic element has as yet escaped detection by the chemist. It is needed in comparatively small quantities, and it might be withheld without detriment for a considerable time. If food were sufficient in kind but deficient in quantity, the result would be complete starvation. When the food is deficient in variety, the result would be incomplete or partial starvation. This partial starvation is an important factor in the production of neuroses. It is most commonly present in infants in the form of fat starvation, which was natural enough when one remembered the very large consumption of commercial infant-foods at the present time. Proteid starvation is much less frequent. In the causation of changes in the tissues there were other factors besides interference with the food-supply, *e. g.*, abnormal nerve-impulses, direct toxic action, and pathologic conditions affecting the organism, such as syphilis and infectious diseases. In conclusion, it was stated that neuroses are always dependent upon abnormal chemic constitution of the tissues, and that such a condition might result from one of three factors, viz: (1) heredity; (2) deficient constructive metabolism, which might follow from insufficient food-supply; (3) deficient destructive metabolism.

DR. ROTCH, of Boston, stated that infants vary greatly in their individual requirements in regard to the quantity of fat needed by the organism.

DR. WILLIAM P. NORTHRUP, of New York City, reported "A Case of Cyanosis," and demonstrated the lesions found in the heart. The patient was a boy, four-and-a-half years old, who, although apparently well nourished,

had pale cheeks and dusky lips and fingers. The digits were knobbed and the belly prominent. Three or four times each week he suffered from very severe attacks of dyspnea, and he also had more or less dyspnea after meals. Palpation showed a fine wave at the left third interspace; there was a soft purring thrill over the base of the heart; the apex-beat was half an inch outside of the left nipple-line; cardiac dulness extended from the right sternal border to the left nipple; there was no dulness to the right of the sternum. A loud, harsh, systolic murmur was heard along the left margin of the sternum, and was distinctly localized at the second left interspace at its junction with the third rib. This murmur was not transmitted to the left or along the aorta. The second sound of the heart was scarcely audible. At the autopsy the right ventricle was found to be markedly hypertrophied, tilting the heart to the left. The apex of the heart was to the outside of the nipple-line, and no part of this organ extended to the right of the sternum. The aorta was distended; the pulmonary artery was unusually small; the ductus arteriosus was impervious; the foramen ovale was practically closed; the left ventricle was normal; the walls of the right ventricle were thicker than those of the left; the septum was incomplete at the auricular extremity; the conus arteriosus was constricted. A probe could not be easily passed from the pulmonary artery into the right ventricle because the pulmonary orifice was at right angles to the axis of the artery, and was very small. The case was one of early fetal endocarditis, giving rise to constriction of the conus arteriosus. This endocarditis must have occurred early in development, before the septum was completed, so that Nature could elect the very best point to effect a compensatory circulation. The aorta, receiving a direct stream of blood from both ventricles, was distended; the pulmonary artery, receiving very little blood, remained small.

DR. T. M. ROCH, the Chairman of the Committee on the Nomenclature of Diseases of the Mouth, then presented the report of the committee. He said that there should be uniformity in the designation of diseases, so that a disease would be called by the same name wherever it was written or spoken about, and that the nomenclature offered by the committee was intended to aid in the accomplishing of this object. The term stomatitis was taken as a basis, and from this four divisions were made: (1) stomatitis catarrhalis; (2) stomatitis herpetica or aphthosa; (3) stomatitis ulcerosa; and (4) stomatitis mycetogenetica. These were again subdivided; for example, stomatitis catarrhalis was subdivided into (1) stomatitis secondary to the exanthemata; and (2) traumatic stomatitis. The report of the committee was accepted by the Society.

The Committee on the Nomenclature of the Diseases of the Gastro-Enteric Tract then presented its report. This consisted of a schematic classification of these diseases. There were two grand divisions—gastric and enteric—and the chief subdivisions were (1) developmental; (2) functional; (3) organic. There were also many subdivisions of each one of these grand divisions. The report was accepted after some discussion.

DR. DILLON BROWN, of New York City, then read a paper entitled "The Early Diagnosis of Pott's Disease of the Spine in Children." He divided the symptoms into three groups: (1) Those common to all cases of tuberculosis, such as peculiar night-cries and evening rise of temperature; (2) the peculiar attitude and rigidity of the spine; (3) the symptoms secondary to interference with the spinal cord and nerves, such as paralysis and dyspnea. In all cases of curvature of the spine the deformity is rounded and not angular. Psoas contraction is almost pathognomonic of spinal caries.

FOURTH DAY—JUNE 1ST.

DR. WILLIAM P. NORTHRUP, of New York City, read a preliminary paper on "Infantile Myxedema," as illustrated by two cases which he reported. The first case occurred in a female child, nine years old, four healthy children having been born previously. The parents were Irish and American, and were strong and healthy. The subject of this report had practically ceased developing at the age of nine months, and when seen, at the age of nine years, was mentally no older than when nine months old. The head was large; the color peculiar, yellowish, and tallow-like; the hair thin; the bridge of the nose characteristically flattened and the nostrils tilted back; the lower lids diffusely swollen; the cheeks pendulous; the lips thick and anemic; the tongue swollen and protruding; the gums filthy and suppurating; the breath emitting a peculiarly characteristic and offensive odor; the soles of the feet dry and parchment-like. The thyroid gland did not seem to be especially enlarged. The only words the child would say were "Dada;" and it would sit for hours at a time tied in a chair. There were some evidences of rickets. For four days after the child was admitted to the hospital no treatment was instituted, in order that the natural course of the temperature might be observed. It was found that the rectal temperature varied between 97.5° and 99.5°. A reliable glycerin extract of the sheep's thyroid was then administered, the doses being so regulated as to keep the rectal temperature just below 100°. Four minims of the extract formed the highest daily dose given. On the fourth day the temperature overstepped the limit, and the thyroid extract was stopped for a short time. Two days later, the breath had become less offensive. While under observation the child had an attack of acute pneumonia, and this necessitated stopping the treatment for a while. The tongue swelled while the thyroid extract was suspended, and again diminished in size on resumption of the treatment. After eighty days of treatment, it was found that the tongue was smaller; the countenance brighter; that the child was better in appearance and more good-natured, and that there was no excessive perspiration. There had, however, been no increase in height and no improvement in the condition of the feet or in the child's speech.

The second case occurred in a boy of twelve years, who presented nothing like so typical an appearance. He rarely spoke, and was very slow even in answering such a question as "What is your name?" He was still under treatment, but so far had not shown any striking improvement.

DR. WILLIAM OSLER, of Baltimore, read a paper on the "Successful Treatment of a Case of Infantile Myxedema with the Thyroid Extract." The patient was three years of age, in appearance a typical cretin, and had been under treatment for more than a year. There was no

enlargement of the thyroid gland. At first the child took an amount corresponding to about one-fourth of a gland in each twenty-four hours. When seen about one month ago it was evident that a remarkable improvement had taken place. The cretinoid aspect had been entirely lost; the flesh was firm and the nutrition much improved; and the mental condition and speech had undergone the most remarkable improvement. Dr. Osler reported a second case, in a cretin nineteen years of age, in which the treatment had been carried out, although not so systematically, and with not very decided benefit.

DR. W. L. CARR, of New York City, spoke of a case he had seen in which very marked improvement in the mental condition and general nutrition had followed the use of the thyroid extract.

DR. FORCHHEIMER, of Cincinnati, spoke of a nineteen-year old cretin in whom treatment with the thyroid extract produced such a condition of excitement, almost maniacal, that it was necessary to stop the thyroid extract.

DR. L. EMMET HOLT, of New York City, read a paper entitled "Acute Pyelitis in Infancy." He reported the case of a male child, eighteen months old, that suddenly became ill, and presented a temperature of 103 5°. There was marked pyrexia for about five days, and examination of the urine showed it to contain a large quantity of pus, but no casts. The pus diminished slowly in quantity, and disappeared at the end of about three weeks. Twenty months later, examination showed that the child was in all respects well, and the parents stated that there had been no return of the urinary symptoms.

A second case was a female child, eight months old, in whom the illness began with a chill and a sudden rise of temperature to 105°. The existence of pneumonia was positively excluded. In this case also there was considerable pus in the urine, and it disappeared slowly under the administration of small doses of potassium citrate. This case likewise ran a course of about three weeks, and sixteen months later it was found that the patient had remained free from all urinary symptoms.

In a third case, after the initial chill and the rise of temperature, there was an irregularly remittent temperature which did not yield, notwithstanding the administration of exceedingly large doses of quinin. The spleen was not enlarged. On the fourth day, there being some slight straining and pain on urination, a specimen of urine was obtained, and examination showed it to be strongly acid and purulent. This case extended over a period of about three months, and presented a number of unusual features. It was possible that there was a malarial element in this case.

DR. SEIBERT said he thought the first case appeared to be one of cystitis. He would like to know upon what Dr. Holt based the differential diagnosis in this case between purulent cystitis and pyelitis; and also on what he based his diagnosis in the other case between purulent inflammation of the kidneys and purulent pyelitis.

DR. OSLER said that he thought malaria could be excluded when there was no enlargement of the spleen and the symptoms did not yield to quinin.

DR. FORCHHEIMER said that he had frequently seen cases of low-grade intermittent fever which did not yield to quinin, and he had also found that in cases of malaria the spleen was not always enlarged.

DR. HOLT said that the absence of mucus, the fact that the urine was always acid, and that there was no evidence of vulvo-vaginitis seemed sufficient to exclude cystitis in the first case. The second case might have been malarial. He agreed with Dr. Osler regarding the points in the diagnosis of malaria.

DR. GEORGE N. ACKER, of Washington, D. C., exhibited a "mirror writer" in the person of a colored boy ten years of age, whose writing was reversed, so that it could be read when reflected in a mirror. This boy was a premature child and was very delicate during his early infancy. He had many convulsions, which became much more frequent and severe after a fall on the head when he was about two or three years of age. He had also had a persistent chorea. He now wrote with equal dexterity with both hands. There was a history of insanity on the mother's side, and the father was markedly neurotic.

NEWS ITEMS.

The Association of American Medical Colleges, at its recent meeting at San Francisco

Resolved, That colleges, members of this Association, shall require of all matriculates an examination as follows: 1. An English composition in the handwriting of the applicant of not less than two hundred words; said composition to include construction, punctuation, and spelling. 2. Arithmetic—Fundamental Rules, Common and Decimal Fractions, and Ratio and Proportion. 3. Algebra, through Quadratics. 4. Physics, Elementary —Gage. 5. Latin—An amount equal to one year's study, as indicated in Harkness' Latin Reader.

(This resolution does not apply to students exempt from the entrance examination, as per Section 2, Article III.)

Resolved, That the following classes of students be recognized as entitled to apply for advanced standing in colleges members of this body: *a*. Such graduates of recognized colleges and universities as have completed the prescribed courses in Chemistry and Biology therein. *b*. Graduates and matriculates of colleges of Homeopathy. *c*. Graduates and matriculates of colleges of Eclectic Medicine. *d*. Graduates and matriculates of colleges of Dentistry requiring two or more courses of lectures before conferring the degree of D.D.S. *e*. Graduates and matriculates of colleges of Pharmacy. *f*. Graduates and matriculates of colleges of Veterinary Medicine.

It is provided, however, that the foregoing class of students be required to comply with the provisions of the entrance examination, and to prove their fitness to advanced standing by an individual examination upon each branch below the class he or she may desire to enter.

Resolved, That students graduating in 1899 or subsequent classes be required to pursue the study of medicine four years and to have attended four annual courses of lectures of not less than six months' duration each.

The American Academy of Medicine will hold its nineteenth annual meeting at Jefferson, N. H., on August 29 and 30, 1894. The following papers may be expected:

"The Retrogressives: What Produces Them; Classification," by Bayard Holmes, Chicago, Ill. "Importance of the Study of the Subject to the Profession," by Chas. McIntire, Easton, Pa. "The Provident Dispensary in England," by H. Webster Jones, London, England. Title to be announced, by J. A. Spalding, Portland, Me. "Assistance and Care for the Blind," by Charles A. Oliver, Philadelphia. "Prevention of Blindness," by Benjamin Lee, Philadelphia. "Present Status of Legislation for the Prevention of Blindness from Infantile Ophthalmia," by Lucien Howe, Buffalo. "Senile Dementia and Testamentary Capacity," by J. N. Whittaker, Cincinnati. Title to be announced, by Gershom H. Hill, Independence, Ia. "What Agencies Conspire to Check Development in the Minds of Children?" by J. Madison Taylor, Philadelphia. "The Medical Service of the U. S. Pension-Bureau," by P. S. Conner, Cincinnati. "Physical Training for Delinquents," by Helen C. Putnam, Providence, R. I. "Government Commission instead of State License," by J. D. Kelly, New Haven, Conn. "The Relation of Food-Adulterations to the Dependent Classes," by Henry Leffmann, Philadelphia. Title to be announced, by G. M. Gould, Philadelphia. President's Address. Title to be announced, by F. H. Gerrish, Portland, Me.

Arrangements have been made for a special excursion from New York and Boston and return at reduced rates. A choice is offered of an eleven days' excursion, including most of the expenses, or of returning immediately at the close of the meeting, and is open to any one desirous of attending the meeting, whether members of the Academy or not. Any additional information about the meeting or the excursion may be obtained from the Secretary of the Academy, Dr. Charles McIntire, Easton, Pa.

The American Public Health Association will hold its twenty-second annual meeting at Montreal, Canada, September 25, 26, 27, 28, 1894. The following topics have been selected for consideration: "The Pollution of Water-supplies." "The Disposal of Garbage and Refuse." "Animal Diseases and Animal Food." "The Nomenclature of Diseases and Forms of Statistics." "Protective Inoculations in Infectious Diseases." "National Health Legislation." "The Cause and Prevention of Diphtheria." "Causes and Prevention of Infant Mortality." "The Restriction and Prevention of Tuberculosis." "Car Sanitation." "The Prevention of the Spread of Yellow Fever."

Papers are invited upon the following additional subjects: "On the Education of the Young in the Principles of Hygiene." "Private Destruction of Household Garbage and Refuse." "Disinfection of Dwellings after Infectious Diseases." "Inspection of School-children with reference to Eyesight."

The Cholera and the Plague.—It is reported that during the first three days of July there were twenty-one cases of cholera in St. Petersburg, six of which terminated fatally. In the twenty-four hours ending at noon on July 4th, eighteen new cases and five deaths were reported. Eighteen cases and four deaths were reported in Cronstadt between July 1st and 4th. A fatal disease bearing strong resemblance to the plague prevailing at Hong Kong is said to be prevalent in numerous Russian provinces, and has also made its appearance in St. Petersburg, Moscow, and other places. From Brussels it is reported that three cases of cholera with one death occurred at Liége on July 5th. Three cases with one death are reported from Angleur, near Liége, and one fatal case from Jeysille.

Gussenbauer, of Prague, has been placed in charge of the second surgical clinic of the University of Vienna. Czerny, of Heidelberg, after having agreed to accept the succession to Billroth, subsequently declined the appointment through an unfortunate misunderstanding.

The American Electro-Therapeutic Association will hold its fourth annual meeting in New York, September 25th, 26th, and 27th, at the Academy of Medicine. Members of the medical profession are cordially invited to attend.

Southern Medical Review is the name of a new "monthly journal of medicine and surgery," published at Houston, Tex., and edited by Dr. N. J. Phenix.

Dr. V. P. Gibney has been elected Professor of Clinical Surgery in the College of Physicians and Surgeons of New York City.

Dr. B. Meade Bolton has been made Associate in Bacteriology in the Johns Hopkins University.

Addendum.—In the case reported by Dr. J. S. Carpenter in THE MEDICAL NEWS of June 23, 1894, p. 700, it should have been stated that the dose of morphin and atropin injected hypodermatically and followed by alarming symptoms was gr. ss. and gr. $\frac{1}{75}$ respectively.

BOOKS AND PAMPHLETS RECEIVED.

Utilisation du Courant de la Rue par un Noveau Moteur-dynamo combiné à une Installation électrique de Cabinet. Par A.-W. de Roaldès, M.D. Extrait des Archives d'Électricité Médicale expérimentales et cliniques.

Cases of Alarming Epistaxis of Grippal Origin, and Dangers of Post-nasal Plugging. By A. W. de Roaldès, M.D. Reprinted from the Medical Record, 1893.

Fibrous Bodies in the Tunica Vaginalis Testis; with a Report of Three Cases. By Thomas S. K. Morton, M.D. Reprinted from the Transactions of the Philadelphia County Medical Society, 1894.

Ueber den therapeutischen Werth des Salols bei der Cholera-Diärrhea. Von M. Wolkowitsch. Sonderabdruck aus Therapeutische Monatshefte, 1893.

The Treatment of Burns. By Thomas S. K. Morton, M.D. Reprinted from the Philadelphia Polyclinic, 1893.

Tumors of the Peripheral Nerves; with Report of a Case of Sarcoma of the Sciatic. By Thomas S. K. Morton, M.D. Pamphlet. 1883.

Early Human Embryos and the Mode of their Preservation. By Franklin P. Mall, M.D. Reprinted from the Johns Hopkins Hospital Bulletin, 1893.

The Surgery of the Hand. By Robert Abbe, M.D. Reprinted from the New York Medical Journal, 1894.

Description of a New Optometer for Correction of Astigmatism by Distant Tests. By Carl Weiland, M.D. Reprinted from the Annals of Ophthalmology and Otology, 1894.

Prevention of Disease. By J. G. Porteous, M.D. Pamphlet. Read before the New York State Medical Association, October 10, 1893.